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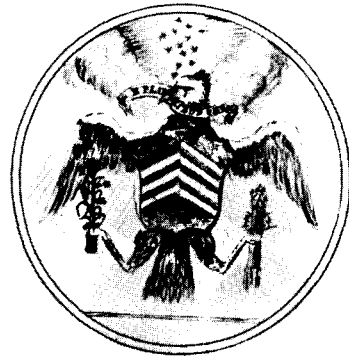
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THE ARTICLES PUBLISHED IN THIS ISSUE of the *Review* are three of the six addresses on "The American Experience" given at the annual meeting of the American Historical Association, December 28–30, 1976 in Washington, D.C. The remaining addresses—by Robert F. Berkhofer, Jr., John M. Murrin, and Timothy L. Smith—are to be published elsewhere.

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America: Experiment or Destiny?

ARTHUR SCHLESINGER, JR.

NEARLY TWO CENTURIES AFTER MICHEL DE CREVECOEUR propounded his notorious question, an American Indian writing in the bicentennial year on the subject "The North Americans" for a magazine directed to American blacks concluded: "No one really knows at the present time what America really is."¹ Surely few observers were more entitled to wonder at the continuing mystery than those who like Vine Deloria, Jr., could accurately claim the designation "Native American." Surely no audience had more right to share the bafflement than one made up of descendants of slaves.

I hardly suppose that our discussions here—dominated as they are by white male historians—will have much greater success in resolving Crevecoeur's perplexity. Indeed, should the American Historical Association decide to discuss the meaning of the American experience again in 2076, we may confidently predict that our tercentennial successors, even if better distributed in sex and color, will be equally impotent before the problem. All any of us can do is descry a figure in the carpet—realizing as we do that contemporary preoccupations define our own definitions. That is all right, so long as we recognize what we are doing. "The one duty we owe to history," Oscar Wilde said with the utmost reasonableness, "is to rewrite it."² Our *AHR* series, like the *AHA* series from which it is drawn, will, I fear, reveal less about the principles of the appointed subject than about the predilections of the appointed participants. But then this is more or less true of all historical writing.

MY EFFORT HERE WILL BE TO SUGGEST TWO THEMES that seem to me to have subsisted in subtle counterpoint since the time when English-speaking white men first began the invasion of America.³ Both themes had their origins in the Calvinist ethos. Both were subsequently reinforced by secular infusions of one sort or another. Both have dwelt within the American mind and struggled for

¹ Vine Deloria, Jr., "The North Americans," reprinted from *Crisis in the Congressional Record*, 94th Cong., 2d Sess. (1976), E2494-95.

² "The Critic as Artist," in Richard Ellman, ed., *The Artist as Critic: The Critical Writings of Oscar Wilde* (New York, 1969), 359.

³ I borrow the phrase, of course, from Francis Jennings's recent and admirable exercise in redressing American history, *The Invasion of America: Indians, Colonialism, and the Cant of Conquest* (Chapel Hill, 1975).

its possession through the course of our history. Their competition will doubtless continue for the rest of the life of the nation. This paper aims to present these rival themes—by necessity, in broad strokes and excessive oversimplifications—and to propose some points about the relationship between the divergent outlooks and the health of the republic.

I will call one theme the tradition and the other the counter-tradition, thereby betraying at once my own bias. Other historians might reverse the terms. I would not quarrel too much about that. Let them betray their own biases. In any event, the tradition, as I prefer to style it, sprang initially from historic Christianity as mediated by Augustine and Calvin. The Calvinist ethos, as we all know, was suffused with convictions of the depravity of man, of the awful precariousness of human existence, of the vanity of mortals under the judgment of a pitiless and wrathful deity. Harriet Beecher Stowe recalled the atmosphere in *Oldtown Folks*: “The underlying foundation of life . . . in New England, was one of profound, unutterable, and therefore unuttered melancholy, which regarded human existence itself as a ghastly risk, and, in the case of the vast majority of human beings, an inconceivable misfortune.”⁴ “Natural men,” cried Jonathan Edwards, “are held in the hand of God, over the pit of hell. . . . The devil is waiting for them, hell is gaping for them, the flames gather and flash about them, and would fain lay hold on them, and swallow them up; the fire pent up in their own hearts is struggling to break out. . . . You have nothing to stand upon, nor any thing to take hold of; there is nothing between you and hell but the air.”⁵ The language rings melodramatically in twentieth-century ears; but perhaps we moderns can more easily accept it as a metaphorical rendering of what those for whom God is dead prefer to call the existential crisis.

So terrible a sense of the nakedness of the human condition turned all of life into an unending and implacable process of testing. “We must look upon our selves,” said William Stoughton, the chief justice of the court that condemned the Salem witches, “as under a *solemn divine Probation*; it hath been and it is a Probation-time, even to this whole people. . . . This hath been and is a time and season of eminent trial to us.”⁶ So had it been at all times for all people. Most had failed the test. Were the American colonists immune to the universal law? In this aspect, the Calvinist notion of “providential history” argued against American exceptionalism. In the Puritan cosmos, Perry Miller has written, “God is not a being of whims and caprices, He is not less powerful at one moment than another; therefore in a certain sense any event is just as significant as any other.”⁷ This facet of the Calvinist outlook came close to the view of the Lutheran Ranke in the nineteenth century that “every epoch is directly before God.”⁸

⁴ (Boston, 1869) 368.

⁵ “Sinners in the Hands of an Angry God.”

⁶ “New England’s True Interest,” in Perry Miller and Thomas H. Johnson, eds., *The Puritans* (1938; reprint ed., New York, 1963), 1: 244.

⁷ Miller and Johnson, *Puritans*, 1: 82–83.

⁸ Friedrich Meinecke, *Historicism* (London, 1972), 505.

The idea of “providential history”⁹ supposed that all secular communities were finite and problematic; all flourished and all decayed; all had a beginning and an end. For Christians this idea had its *locus classicus* in Augustine’s great attempt to solve the problem of the decline and fall of Rome—the problem that more than any other transfixed the serious historical minds of the West for precisely thirteen hundred and fifty years after the appearance of *The City of God*. This obsession with the classical catastrophe provided a link between the sacred and the profane in the American colonies—between seventeenth-century Americans who read the Christian fathers and eighteenth-century Americans who read Polybius, Plutarch, Cicero, Sallust, Tacitus. By the time the revolutionaries came to Philadelphia in 1776, the flames of Calvinism were already burning low. Hell was dwindling into an epithet. Original sin, not yet abandoned, was like everything else, secularized. Still, for the fathers of the republic as for the fathers of the Church, the history of Rome, in the words of Jaroslav Pelikan, remained the “textbook to which to turn for instruction about the course of human affairs, the development of freedom and the fate of despotism.”¹⁰

From different premises, Calvinists and classicists reached the same conclusion about the fragility of human striving. Antiquity haunted the Federal imagination. Robert Frost’s poem about “the glory of a next Augustan age. . . . A golden age of poetry and power” would have been more widely understood at George Washington’s inauguration than at John Kennedy’s. The Founding Fathers had embarked on a singular adventure—the adventure of a *republic*. For landmarks on a perilous voyage they peered across the gulf of centuries to Greece and especially to Rome, seeking the first and noblest expression of free men aspiring to govern themselves. “The Roman republic,” Alexander Hamilton wrote in the *Federalist*, “attained to the utmost height of human greatness.”¹¹ In this conviction the first generation of the American republic designed its buildings, wrote its epics, called the upper chamber of its legislature the Senate, signed its greatest political treatise “Publius,” sculpted its heroes in togas, baptized new communities, organized the Society of the Cincinnati, and instructed the young. “One is hagridden,” complained Edmund Trowbridge Dana in 1805, “. . . with nothing but the classicks, the classicks, the classicks!” (As a consequence of this heretical attitude, Dana failed to graduate from Harvard, receiving posthumously eighty years after an AB degree as of the class of 1799.¹²)

There was plausibility in the parallel. Alfred North Whitehead later recalled only two occasions in history “when the people in power did what needed to be done about as well as you can imagine its being possible”—the

⁹ See Sacvan Bercovitch, *The Puritan Origins of the American Self* (New Haven, 1975), 41.

¹⁰ “The Lessons of History,” paper delivered at a conference on “The Nature of a Humane Society” sponsored by the Southeastern Pennsylvania Synod of the Lutheran Church in America, October 29, 1976, 1.

¹¹ Number 34.

¹² Dana’s article, “The Winter of Criticism,” appeared in the *Monthly Anthology and Boston Review*, 12 (October 1805), in Lewis P. Simpson, ed., *The Federalist Literary Mind* (Baton Rouge, 1962), 209, 230.

age of Augustus and the framing of the American Constitution.¹³ There was also warning. For the grandeur that was Rome had come to an inglorious end. Could the United States of America hope to do better?¹⁴

The Founding Fathers passionately ransacked the classical historians for ways to escape the classical fate. One cannot easily overstate the anxiety that attended this search or the relevance they seemed to find in the writers of antiquity. Thomas Jefferson thought Tacitus "the first writer of the world without a single exception. His book is a compound of history and morality of which we have no other example." "To live without having a Cicero and a Tacitus at hand," said John Quincy Adams, a founding son, "seems to me as if it was a privation of one of my limbs."¹⁵ As Adams's cousin William Smith Shaw put it, "The writings of Tacitus display the weakness of a falling empire and the morals of a degenerate age. . . . They form the subject of deep meditation for all statesmen who wish to raise their country to glory; to continue it in power, or preserve it from ruin."¹⁶ Polybius was almost as crucial—for delineating the cycle of birth, growth, and decay that constituted the destiny of states; and for sketching the mixed constitution with balanced powers that the Founding Fathers seized as a glimpse at remedy.¹⁷

The classical indoctrination reinforced the Calvinist judgment that this was a time of probation for America. For the history of antiquity did not teach the inevitability of progress. It proved rather the perishability of republics, the subversion of virtue by power and luxury, the transience of glory, the mutability of human affairs. The conventional emphasis on the benign John Locke as the father of us all obscures the dark and ominous strain in the thought of the Founders recently recalled to our attention by J. G. A. Pocock—the strain that led from Machiavelli's *Discourses on Livy* through Harrington and Montesquieu to the Constitutional Convention.¹⁸

¹³ Lucien Price, ed., *Dialogues of Alfred North Whitehead* (Boston, 1954), 161, 203. Perry Miller agrees: "As far as I read the history of the West, I find only one other great civilization that faced an analogous predicament, and that was the Roman Empire." "The Shaping of the American Character," *New England Quarterly*, 28 (1955): 439.

¹⁴ The careful reader will note that some of the Founders' allusions are to Rome as a republic, some to Rome as an empire. It is not clear that they drew too sharp a distinction between these phases in Roman history. See, for example, Fisher Ames: "Rome was a republic from its very birth. It is true, for two hundred and forty-four years it was subject to its kings; but the spirit of liberty was never more lofty at any period of its long troubled life than when Rome was governed by kings. They were, in war, generals; in peace, only magistrates. For seven hundred years Rome remained a republic." Seth Ames, ed., *The Works of Fisher Ames* (Boston, 1854), 2: 332–33.

¹⁵ In W. O. Clough, ed., *Intellectual Origins of American National Thought* (1955; reprint ed., New York, 1961), 71.

¹⁶ "The Age of Tacitus," in the *Monthly Anthology*, 4 (July 1807): 368, in Simpson, *Federalist Literary Mind*, 50.

¹⁷ Polybius was "diligently read in America especially during the Revolutionary period." Meyer Reinhold, ed., *The Classic Pages: Classical Reading of Eighteenth-Century Americans* (University Park, Pa., 1975), 121. Also see Richard M. Gummere, *The American Colonial Mind and the Classical Tradition* (Cambridge, Mass., 1963), *passim*.

¹⁸ Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975). The same point has been made with less elaboration by Gordon S. Wood, *The Creation of the American Republic, 1776–1787* (Chapel Hill, 1969), and by Gerald Stourzh, *Alexander Hamilton and the Idea of Republican Government* (Stanford, 1970). Reinhold writes that Polybius was known in America partly through direct study of Book VI of his *History* and "partly as mediated through Machiavelli's *Discourses* and Montesquieu's *Laws*." *Classic Pages*, 121.

Pocock isolates what he called “the Machiavellian moment”—the moment in which a republic is “seen as confronting its own temporal finitude, as attempting to remain morally and politically stable in a stream of irrational events conceived as essentially destructive of all systems of secular stability.”¹⁹ He argues persuasively that this apprehension of the mortality of states was a vital element in the sensibility of Philadelphia in 1787. Not only was man vulnerable through this propensity to sin, but republics were vulnerable through their propensity for corruption. The dread of corruption, as Bernard Bailyn has demonstrated, had long since been imported from England.²⁰ History showed that, in the unceasing contest between corruption and virtue, corruption had always—up at least to 1776—triumphed.

The Founding Fathers had an intense conviction of the improbability of their undertaking. Such assets as they possessed came in their view from geographic and demographic advantage, not from divine intercession. Benjamin Franklin ascribed the inevitability of American independence to such mundane factors as population increase and vacant lands, not to providential design.²¹ But even these assets could not be counted on to prevail against history and human nature. Hamilton said in the New York ratifying convention, “The tendency of things will be to depart from the republican standard. This is the real disposition of human nature.” By 1802, when the Constitution was fifteen years old, he pronounced it a “frail and worthless fabric.” “Every republic at all times,” he said (always the classical analogy), “has its Catilines and its Caesars. . . . If we have an embryo-Caesar in the United States, ’tis Burr.”²² Jefferson and John Adams no doubt thought it was Hamilton.

If Hamilton be discounted as a temperamental pessimist or a disaffected adventurer, his great adversaries were not always more sanguine about the republic’s future. “Commerce, luxury, and avarice have destroyed every republican government,” Adams wrote Benjamin Rush in 1808. “We mortals cannot work miracles; we struggle in vain against the constitution and course of nature.”²³ “I tremble for my country,” Jefferson had said in the 1780s, “when I reflect that God is just.”²⁴ Though he was trembling at this point—rightly and presciently—over the problem of slavery, he also trembled chronically in the nineties over the unlikely prospect of “monarchy.” In 1798 he saw the Alien and Sedition Acts as tending to drive the states “into revolution and blood, and [to] furnish new calumnies against Republican Government, and new pretexts for those who wish it to be believed, that men cannot be governed but by a rod of iron.”²⁵ As president Jefferson trembled himself into unworthy panic over the murky dreams of Aaron Burr, that embryo forever

¹⁹ *Machiavellian Moment*, viii.

²⁰ *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967).

²¹ See Joseph Ellis, “Habits of Mind and an American Enlightenment,” *American Quarterly*, 28 (1976): esp. 161.

²² Quoted in Stourzh, *Alexander Hamilton*, 71, 98; and Hamilton to Gouverneur Morris, February 27, 1802 in Hamilton, *Works*, ed. H. C. Lodge (New York, 1904), 7: 591.

²³ Adams to Rush, September 27, 1808 in Adrienne Koch and William Peden, eds., *The Selected Writings of John and John Quincy Adams* (New York, 1946), 149–50.

²⁴ *Notes on Virginia*, Query XVIII.

²⁵ Kentucky Resolutions of 1798, IX.

struggling to become Caesar. From the next generation William Wirt asked in 1809, "Can any man who looks upon the state of public virtue in this country . . . believe that this confederated republic is to last forever?"²⁶

THIS PERVASIVE SELF-DOUBT, this urgent sense of the precariousness of the national existence, was no doubt nourished by European assessments of the American prospect. For eminent and influential Europeans regarded the new world, not as an idyll of Lockean felicity—"in the beginning, all the world was America"²⁷—but as a disgusting scene of degeneracy and impotence.

In the middle of the eighteenth century the famous Georges Buffon lent the weight of scientific authority to the proposition that life in the western hemisphere was consigned to biological inferiority. American animals were smaller and weaker; European animals shrank when transported across the Atlantic except, Buffon specified, for the fortunate pig. As for the indigenous natives of the fallen continent, they too were small and weak, passive and backward. Soon Abbé de Pauw converted Buffon's pseudoscience into derisive polemic. In England Oliver Goldsmith portrayed a gray and gloomy land where no dogs barked and no birds sang. Horace Walpole drew the inevitable conclusion: "Buffon says, that European animals degenerate across the Atlantic; perhaps its migrating inhabitants may be in the same predicament."²⁸ As William Robertson, the Historiographer Royal for Scotland, rendered it in his widely read *History of America*, published the year after the Declaration of Independence, "The same qualities in the climate of America which stunted the growth . . . of its native animals proved pernicious to such as have migrated into it voluntarily."²⁹

No one made this case more irritatingly and perseveringly than Abbé Raynal in France. Buffon, Jefferson observed, had never quite said that the European degenerated in America: "He goes indeed within one step of it, but he stops there. The Abbé Raynal alone has taken that step."³⁰ Raynal's much-reprinted work, *Philosophical and Political History of the Settlements and of the Commerce of Europeans in the Two Indies*, first published in 1770, explained how European innocence was under siege by American depravity. America, Raynal wrote, had "poured all the sources of corruption on Europe." The search for American riches brutalized the European intruder. The climate and soil of America caused the European species, human as well as animal, to deteriorate. "The men have less strength and less courage . . . and are but little susceptible of the lively and powerful sentiment of love"—a comment that perhaps revealed Raynal as in the end more a Frenchman than as an abbé. "Let me stop here," Raynal said in summation,

²⁶ Wirt to Benjamin Edwards, December 22, 1809 in J. P. Kennedy, ed., *Memoirs of the Life of William Wirt* (New York, 1849), 1: 246-47.

²⁷ *Second Treatise on Civil Government*, ch. 2, par. 49.

²⁸ Quoted in Antonelli Gerbi, *The Dispute of the New World: The History of a Polemic, 1750-1900* (Pittsburgh, 1973), ch. 1, 160-75.

²⁹ Quoted in Henry Steele Commager, *Jefferson, Nationalism, and the Enlightenment* (New York, 1975), 43.

³⁰ Quoted in Gerbi, *Dispute*, 262.

and consider ourselves as existing at the time when America and India were unknown. Let me suppose that I address myself to the most cruel of Europeans in the following terms. There exist regions which will furnish thee with rich metals, agreeable clothing, and delicious food. But read this history, and behold at what price the discovery is promised to thee. Does thou wish or not that it should be made? Is it to be imagined that there exists a being infernal enough to answer this question in the affirmative! *Let it be remembered, that there will not be a single instant in futurity, when my question will not have the same force.* [Emphasis added.]

After the Declaration of Independence, Raynal added insult to injury. He was passing through Lyons on a journey from Paris to Geneva. The local academy, apprised of his presence, made him a member. In return, Raynal gratefully established a prize of 1200 francs to be awarded by the Academy of Lyons for the best essay on the arresting topic: "Was the discovery of America a blessing or a curse to mankind? If it was a blessing, by what means are we to conserve and enhance its benefits? If it was a curse, by what means are we to repair the damage?"³¹

The Founding Fathers were highly sensitive to the proposition that America was a mistake. Franklin, who thought Raynal an "ill-informed and evil-minded Writer," once endured a monologue by the diminutive abbé on the inferiority of the Americans at his own dinner table in Paris. "Let us try this question by the fact before us," said Franklin, calling on his guests to stand up and measure themselves back to back. "There was not one American present," wrote Jefferson who was also there, "who could not have tost out of the Windows any one or two of the rest of the Company."³² Jefferson himself devoted long passages in his *Notes on Virginia* to the refutation of Buffon on animals and of Raynal on human beings. Europeans "admired as profound philosophers," Hamilton wrote scornfully in the *Federalist*, "have gravely asserted that all animals, and with them the human species, degenerate in America—that even dogs cease to bark after having breathed a while in our atmosphere."³³ Tom Paine joined the fight; and John Adams noted in his *Defence of the Constitutions of the United States* his delight in the way Paine had "exposed the mistakes of Raynal, and Jefferson those of Buffon, so unphilosophically borrowed from the despicable dreams of De Pau [*sic*]."³⁴

Though the Founders were both indignant and effective in their rebuttal, the nature of the attack could hardly have increased their confidence in the future of their adventure. The European doubt, along with the Calvinist judgment and the Machiavellian moment, made them acutely aware of the chanciness of an extraordinary enterprise. From the fate of the Greek city-states and the fall of the Roman Empire they drew somber conclusions about the prospects of the American republic. They had no illusions about the inviolability of America to history, supposing all states, including the American,

³¹ In Henry Steele Commager and Elmo Giodanetti, eds., *Was America a Mistake? The Eighteenth-Century Controversy* (New York, 1967), 126, 129, 138, 16.

³² Quoted in Gerbi, *Dispute*, 240–42.

³³ Number 11. In a footnote Hamilton cited de Pauw's *Recherches Philosophiques sur les Americains*.

³⁴ *Defence*, Preface.

immediate unto history, as a consistent Calvinist should have supposed all states immediate unto God. "Have we not already seen enough," wrote Hamilton, "of the fallacy and extravagance of those idle theories which have amused us with promises of an exemption from the imperfections, weaknesses, and evils incident to society in every shape? Is it not time to awake from the deceitful dream of a golden age, and to adopt as a practical maxim for the direction of our political conduct that we, as well as the other inhabitants of the globe, are yet remote from the happy empire of perfect wisdom and perfect virtue?"³⁵ Though few were Calvinists of the old school, the Founding Fathers had no illusions about the perfectibility of man, Americans or others. The Constitution, James Bryce has well said, was "the work of men who believed in original sin and were resolved to leave open for transgressors no door which they could possibly shut."³⁶

We have all applied the phrase "end of innocence" to one or another stage of American history. This is surely an amiable flourish—or a pernicious delusion. No people who systematically enslaved black men and killed red men could be innocent. No people reared on Calvin and Tacitus, on Edwards and the *Federalist*, could be innocent. No nation founded on invasion, conquest, and slaughter could be innocent. No state established by revolution and thereafter rent by civil war could be innocent. The Constitution was hardly the product of immaculate conception. The Founding Fathers were not a band of saints. They were brave and imperturbable realists who committed themselves, in defiance of the available lessons of history and theology, to a monumental gamble.

Their recognition of this is why Hamilton, in the third sentence of the first *Federalist*, formulated the issue as he did. The American people, he wrote, had the opportunity "by their conduct and example, to decide the important question, whether societies of men are really capable or not of establishing good government from reflection and choice, or whether they are forever destined to depend for their political constitutions on accident and force." So Washington defined it in his first inaugural address: "The preservation of the sacred fire of liberty and the destiny of the republican model of government are justly considered, perhaps, as *deeply*, as *finally*, staked on the experiment intrusted to the hands of the American people." The Founding Fathers saw the American republic not as a consecration but as the test against history of a hypothesis. They "looked upon the new federal organization," Woodrow Wilson wrote, "as an experiment, and thought it likely it might not last."³⁷

Washington's early successors, with mingled anxiety and hope, reported on the experiment's fortunes. In his last message to Congress, James Madison permitted himself "the proud reflection that the American people have reached in safety and success their fortieth year as an independent nation." This, the presidents believed, had more than local significance. "Our in-

³⁵ *Federalist*, Number 6.

³⁶ *The American Commonwealth* (New York, 1888), 1: 299.

³⁷ Woodrow Wilson, *Constitutional Government in the United States* (1908; reprint ed., New York, 1961), 44–45.

stitutions,” said James Monroe in his last message, “form an important epoch in the history of the civilized world. On their preservation and in their utmost purity everything will depend.” Washington, said Andrew Jackson in his own Farewell Address, regarded the Constitution “as an experiment” and “was prepared to lay down his life, if necessary, to secure it a full and a fair trial. The trial has been made. It has succeeded beyond the proudest hopes of those who framed it.” Still Jackson discerned threats to the experiment—in the “moneyed power” and even more in the dissolution of the union itself, where chaos, he supposed, might lead the people “to submit to the absolute domination of any military adventurer and to surrender their liberty for the sake of repose.”³⁸

Nevertheless, confidence—or at least the simulation of confidence—grew. “The present year,” Martin Van Buren said in 1838, “closes the first half century of our Federal institutions. . . . It was reserved for the American Union to test the advantages of a government entirely dependent on the continual exercise of the popular will.” “After an existence of near three-fourths of a century as a free and independent Republic,” said James Polk in the next decade, “the problem no longer remains to be solved whether man is capable of self-government. The success of our admirable system is a conclusive refutation of the theories of those in other countries who maintain that a ‘favored few’ are born to rule and that the mass of mankind must be governed by force.” The Mexican War, Polk soon added, “evinces beyond any doubt that a popular representative government is equal to any emergency.” Sixty years after the Constitution, Zachary Taylor pronounced the United States of America “the most stable and permanent Government on earth.”³⁹

How is one to account for this rising optimism? It was partly a tribute, reasonable enough, to survival. It was partly the spread-eagleism and vain-glory congenial to a youthful nationalism. It was no doubt also in part admonitory exhortation—let us not throw away what we have so precariously achieved—for the presidents of the middle period must have known in their bones that the American experiment was confronting its fiercest internal trial. No one understood more profoundly the chanciness of the adventure than the young man who spoke in 1838 on “The Perpetuation of our Political Institutions” before the Young Men’s Lyceum of Springfield, Illinois. Over most of the first half century, Abraham Lincoln said, America had been felt “to be an undecided experiment; now, it is understood to be a successful one.” But success contained its own perils; “with the catching, end the pleasures of the chase.” As the memory of the Revolution receded, the pillars of the temple of liberty were crumbling away. “That temple must fall, unless we . . . supply their places with other pillars, hewn from the solid quarry of sober reason.” The conviction of the incertitude of life informed his presidency—and explained its greatness. His first message to Congress asked whether all repub-

³⁸ J. D. Richardson, comp., *Messages and Papers of the Presidents* (Washington, 1909), 1: 579; 2: 262; 3: 295–96, 303.

³⁹ *Ibid.*, 3: 483–84; 4: 532–33, 632; 5: 9.

lics had an “inherent and fatal weakness.” At the Gettysburg cemetery he described the great civil war as “testing” whether any nation conceived in liberty and dedicated to the proposition that men are created equal “can long endure.”⁴⁰

THIS WAS, I TAKE IT, A DOMINANT THEME of the early republic—the idea of America as an experiment, undertaken in defiance of history, fraught with risk, problematic in outcome. But a counter-tradition was also emerging—and, as the mounting presidential optimism suggests, with accumulating momentum. The counter-tradition too had its roots in the Calvinist ethos.

Historic Christianity embraced two divergent thoughts: that all people were immediate unto God; and that some were more immediate than others. At first, Calvin had written in the *Institutes*, God “chose the Jews as his very own flock”; the “covenant of salvation . . . belonged only to the Jews until the wall was torn down.”⁴¹ Then, with what Jonathan Edwards called “the abolishing of the *Jewish dispensation*,” the wall was “broken down to make way for the more extensive success of the gospel.”⁴² The chosen people thereafter were the elect as against the reprobate. Soon the idea of the saints acting within history disappeared into the transcendency of the posthistorical City of God.

So Augustine set along side of “providential history”—the rise and decline of secular communities within history—the idea of “redemptive history”—the journey of the elect to salvation beyond history. The age that sent the Calvinists to New England also saw a revival of the primitive chiliasm of the first century. The New Englanders felt they had been called from hearth and home to endure unimaginable rigor and ordeal in a dangerous land; so they supposed someone of importance had called them, and for important reasons. Their very tribulations seemed proof of a role in redemptive history. “God hath covenanted with his people,” said Increase Mather, “that sanctified afflictions shall be their portion. . . . The usual method of divine Providence [is] by the greatest Miseries to prepare for the greatest Mercies. . . . *Without doubt, the Lord Jesus hath a peculiar respect unto this place, and for this people.*”⁴³

It was not only that they were in John Winthrop’s words, as a City upon a Hill, with the eyes of all people upon them. It was that they had been despatched to New England, as Edward Johnson said, by a wonder-working Providence because “this is the place where the Lord will create a new Heaven, and a new Earth.” The “Lord Christ” intended “to make his New England Souldiers the very wonder of this Age.”⁴⁴ The Reverend Arthur Dimmesdale’s last sermon, Nathaniel Hawthorne tells us, dealt with “the relation between the Deity and the communities of mankind, with a special reference to the New England which they were here planting in the wilder-

⁴⁰ *Collected Works*, ed. R. P. Basler (New Brunswick, N.J., 1953), 1: 113–15; 7: 17.

⁴¹ John Dillenberger, ed., *John Calvin: Selections from His Writings* (New York, 1971), 350, 564.

⁴² *A History of the Work of Redemption*, sec. 3: 1.

⁴³ Quoted in Bercovitch, *Puritan Origins*, 41–42, 54–55.

⁴⁴ In Miller and Johnson, *Puritans*, 1: 145, 152, 199.

ness.” But, where the Jewish prophets had seen ruin for their country, Dimmesdale’s mission was “to foretell a high and glorious destiny for the newly gathered people of the Lord.”⁴⁵ The great Edwards concluded that “the Latter-Day Glory is probably to begin in America.”⁴⁶

This geopolitical specification of the millennium—this identification of the New Jerusalem with a particular place and people—was rare, even in a time of millennial fervor. “What in England, Holland, Germany and Geneva,” Bercovitch writes, “was an *a priori* antithesis [between the saints and the state] became in America the twin pillars of a unique federal eschatology.” For the old world was steeped in iniquity, one more shameful episode in the long shame of providential history. The fact that God had withheld America so long—until the Reformation purified the church, until the invention of printing spread the Bible among the people—argued that He had been preparing it for some ultimate manifestation of His grace. God, said Winthrop, having “smitten all the other Churches before our eyes,” had reserved America for those whom He meant “*to save out of this generall callamitie*,” as he had once sent the ark to save Noah. The new land was certainly a part, perhaps the climax, of redemptive history; America was divine prophecy fulfilled.⁴⁷

The covenant of salvation, it seemed, had passed across the Atlantic. Like original sin, this proposition underwent secularization in the eighteenth century. Before the Revolution, John Adams, reading the original draft of his “Dissertation on the Canon and Feudal Law” to a club of Boston lawyers, indulged himself in that well-known rhetorical flight: “I always consider the settlement of America with reverence, as the opening of a grand scene and design in Providence for the illumination of the ignorant and the emancipation of the slavish part of mankind all over the earth.” On reflection Adams evidently considered the sentiment extravagant if not dubious, for he deleted it from the paper as published. His son John Quincy Adams commented later: “Who does not now see that the accomplishment of this great object is already placed beyond all possibility of doubt?” His grandson Charles Francis Adams called the passage his grandfather cut from the paper “the most deserving of any to be remembered.”⁴⁸ So within a single family the idea of experiment began to yield to a different idea—the idea of an American national destiny.

The achievement of independence gave new status to the theory of America as an “elect nation” (Bercovitch) or a “redeemer nation” (E. L. Tuveson),⁴⁹ entrusted by the Almighty with the charge of carrying its light to the unregenerate world. The Reverend Timothy Dwight, Jonathan Edwards’s grandson, called Americans “this chosen race.”⁵⁰ “God’s mercies to New England,”

⁴⁵ *The Scarlet Letter*, ch. 23.

⁴⁶ *Thoughts Concerning the Present Revival of Religion in New England*, pt. 2, sec. 2.

⁴⁷ Obviously this discussion draws heavily on the brilliant analysis in Bercovitch, *Puritan Origins*, 89–90, 100–04.

⁴⁸ In Charles Sumner, *Prophetic Voices Concerning America* (Boston, 1874), 54–55.

⁴⁹ *Redeemer Nation: The Idea of America’s Millennial Role* (Chicago, 1968).

⁵⁰ Quoted in A. K. Weinberg, *Manifest Destiny* (1935; reprint ed., New York, 1963), 40.

wrote Harriet Beecher Stowe, daughter of one minister and wife of another, foreshadowed “the glorious future of the United States . . . commissioned to bear the light of liberty and religion through all the earth and to bring in the great millennial day, when wars should cease and the whole world, released from the thralldom of evil, should rejoice in the light of the Lord.”⁵¹

Patriotic fervor swept the idea of divine national mission far beyond the evangelical community. Jefferson thought the Great Seal of the United States should portray the children of Israel led by a pillar of light.⁵² “Here Paradise anew shall flourish,” wrote Philip Freneau in an early statement of the myth of American innocence,

by no second Adam lost
No dangerous tree or deathful fruit shall grow,
No tempting servant to allure the soul
From native innocence. . . .⁵³

“We Americans,” wrote the youthful Herman Melville, “are the peculiar, chosen people—the Israel of our time; we bear the ark of the liberties of the world. . . . God has predestinated, mankind expects, great things from our race; and great things we feel in our souls. The rest of the nations must soon be in our rear. . . . Long enough have we been sceptics with regard to ourselves, and doubted whether, indeed, the political Messiah had come. But he has come in *us*.”⁵⁴

The belief that Americans were a chosen people did not imply a sure and tranquil journey to salvation. As the Bible made amply clear, chosen people underwent the harshest trials and assumed the most grievous burdens. The rival propositions—America as experiment, America as destiny—thus shared a belief in the process of testing. But one tested works, the other faith. So Lincoln and Mrs. Stowe agreed from different standpoints in seeing the Civil War as the climactic test. Northern victory, however, strengthened the conviction of providential appointment. “Now that God has smitten slavery unto death,” Mrs. Stowe’s brother Edward wrote in 1865, “he has opened the way for the redemption and sanctification of our whole social system.”⁵⁵

The Kingdom of God was deemed both imminent in time and immanent in America. It was a short step from the Social Gospel at home to Americans carrying the Social Gospel to the world. The Hebrews, the Greeks, and the Romans, wrote the Reverend Josiah Strong, had ably but separately developed the spiritual, intellectual, and physical qualities of man. “Now for the first time in the history of mankind the three great strands pass through the fingers of one predominant race to be braided into a single supreme civilization in the new era, the perfection of which will be the Kingdom fully

⁵¹ Quoted in Bercovitch, *Puritan Origins*, 87–88.

⁵² Quoted in Gilbert Chinard, *Thomas Jefferson: The Apostle of Americanism* (1929; reprint ed., Ann Arbor, Mich., 1957), 428.

⁵³ Quoted in Commager, *Enlightenment*, 188.

⁵⁴ *White-Jacket*, ch. 36.

⁵⁵ Quoted in H. Richard Niebuhr, *The Kingdom of God in America* (1935; reprint ed., New York, 1959), 157.

come. . . . All unite in the one Anglo-Saxon race, indicating that this race is pre-eminently fitted, and therefore chosen of God, to prepare the way for the full coming of his kingdom in the earth."⁵⁶ It was another short step from this to what the Reverend Alexander Blackburn, who had been wounded at Chickamauga, called in 1898 "the imperialism of righteousness";⁵⁷ and from Blackburn to the messianic demagoguery of Albert J. Beveridge, "God has not been preparing the English-speaking and Teutonic peoples for a thousand years for nothing but vain and idle self-contemplation. . . . And of all our race He has marked the American people as His chosen nation to finally lead in the regeneration of the world."⁵⁸

So the impression developed that in the United States of America the Almighty had contrived a nation unique in its virtue and magnanimity, exempt from the motives that governed all other states. "America is the only idealistic nation in the world," said Wilson on his pilgrimage to the West in 1919. "The heart of this people is pure. The heart of this people is true. . . . It is the great idealistic force of history. . . . I, for one, believe more profoundly than in anything else human in the destiny of the United States. I believe that she has a spiritual energy in her which no other nation can contribute to the liberation of mankind. . . . [In the war] America had the infinite privilege of fulfilling her destiny and saving the world."⁵⁹

In another forty years the theory of America as the savior of the world received the furious *imprimatur* of John Foster Dulles, another Presbyterian elder, and from there the country roared on to the horrors of Vietnam. So the hallucination brought the republic from the original idea of America as exemplary experiment to the recent idea of America as mankind's designated judge, jury and executioner. Nor is there strong reason to suppose that Vietnam has altogether cured us of this infatuation. "Call it mysticism if you will," Governor Ronald Reagan said several months ago, "but I believe God had a divine purpose in placing this land between the two great oceans to be found by those who had a special love of freedom and the courage to leave the countries of their birth."⁶⁰ Nor are we yet absolutely clear that the victor in the bicentennial election may not believe that nations, like presidents, may be born again.

WHY DID THE CONVICTION of the corruptibility of men and the vulnerability of states—and the consequent idea of America as experiment—give way to the myth of innocence and the delusion of a sacred mission and a sanctified destiny? The original conviction was rooted in realistic conceptions of history and of human nature—conceptions that waned as the republic prospered.

⁵⁶ *The New Era* (New York, 1893), 71, 354.

⁵⁷ From the *Chicago Standard*, August 6, 1898, in J. W. Pratt, *Expansionists of 1898* (1935; reprint ed., New York, 1964), 293.

⁵⁸ In Norman A. Graebner, ed., *Ideas and Diplomacy* (New York, 1964), 372–73.

⁵⁹ Phrases from speeches in Omaha, Sioux Falls, San Francisco, San Diego, Cheyenne, in Wilson, *Messages and Papers*, ed. Albert Shaw (New York, 1924), 2: 815, 822, 969, 1025, 1086.

⁶⁰ *New York Times*, April 1, 1976.

The intense historical-mindedness of the Founding Fathers did not endure. Though the first generation came to Philadelphia loaded down with historical examples and memories, its function was precisely to liberate its descendants from history. Once the Fathers had done their work, history began again on a new foundation and in American terms. "The Past," Melville said in *White-Jacket*, "is dead, and has no resurrection; but the Future is endowed with such a life that it lives to us even in anticipation."⁶¹ The process of narcissistic withdrawal from history, much commented on by foreign travelers, was sustained by the simultaneous withdrawal, after 1815, from the power embroilments of the old world. The new nation was largely populated by people torn from, fleeing from, or in revolt against their own histories. This also helped take the republic out of the movement and motive of secular history. "Probably no other civilised nation," said the *Democratic Review* in 1842, "has . . . so completely thrown off its allegiance to the past as the American."⁶²

Teleological isolation survived the end of political and economic isolation. We find ourselves at this Bicentennial, for all the show-business clatter of the Fourth-of-July celebrations, an essentially historyless people. Businessmen agree with the elder Henry Ford that history is bunk. The young no longer study history. Intellectuals turn their backs on history in the enthusiasm for the ahistorical behavioral "sciences." As the American historical consciousness has thinned out, the messianic hope has flowed into the vacuum. And, as Christianity turned liberal, shucking off such cardinal doctrines as original sin, one more impediment was removed to belief in national virtue and perfectibility. Experiment gave ground to destiny as the premise of national life.

All this, of course, was both provoked and fortified by latter-day exertions of national power. All nations succumb to fantasies of innate superiority. When they act on these fantasies, as the Spanish did in the sixteenth century, the French in the seventeenth, the English in the eighteenth, the Germans and Japanese and Russians and Americans in the twentieth, they tend to become international menaces. The American hallucination took root during the long holiday from the world of reality. When America re-entered that world, the hallucination was confirmed by the overwhelming power in our own possession.

So the theory of the elect nation, the redeemer nation, the happy empire of perfect wisdom and perfect virtue, almost became the official creed. Yet, while the counter-tradition prospered, the tradition did not quite expire. Some continued to regard it all as the deceitful dream of a golden age, wondering perhaps why the Almighty should have chosen the Americans.⁶³ "The Al-

⁶¹ *White-Jacket*, ch. 34.

⁶² Quoted in R. W. B. Lewis, *The American Adam* (Chicago, 1955), 159.

⁶³ "How odd
Of God
To choose
The Jews"—Hilaire Belloc.

mighty,” Lincoln insisted at his second inaugural, “has His own purposes.” He clearly knew what he was saying, because he wrote soon thereafter to a fellow ironist, Thurlow Weed: “Men are not flattered by being shown that there has been a difference of purpose between the Almighty and them. To deny it, however, . . . is to deny that there is a God governing the world.”⁶⁴

After the war, Walt Whitman, once the supreme poet of democratic faith, suddenly perceived a dark and threatening future. The experiment was in jeopardy. These States, no longer a sure thing, had become a “battle, advancing, retreating, between democracy’s convictions, aspirations, and the people’s crudeness, vice, caprices.” America, Whitman apprehended, might well “prove the most tremendous failure of time.”⁶⁵ “’Tis a wild democracy,” Emerson said in his last public address; “the riot of mediocrities and dishonesties and fudges.”⁶⁶ There is prosopographical felicity in the fact that a fourth generation of Adamses raised particularly keen doubts whether Providence in settling America had after all opened a grand design to emancipate mankind. Henry Adams began as a connoisseur of political ironies, seeing history (in terms well defined by Pocock) as “an intelligible story of how men’s actions produce results other than those they intended.”⁶⁷ Irony was at least at odds with destiny; but in time his brother Brooks pushed Henry, and history too, beyond irony into catastrophe. “You Americans believe yourselves to be exempted from the operation of general laws,” the cynical Baron Jacobi had growled in Henry’s *Democracy*.⁶⁸ Brooks was a mighty exception. Juggling equations of energy, centralization, and social velocity, he doubted whether any society was exempt from the law of civilization and decay.

Henry, seizing his brother’s clue, sought to pursue thought “to the limit of its possibilities,” a point he thought might arrive in the year 1921.⁶⁹ He became a sort of reverse millennialist, convinced that science and technology were rushing the planet toward an apocalypse unredeemed by a Day of Judgment. “At the rate of increase of speed and momentum, as calculated on the last fifty years,” he wrote Brooks in 1901, “the present society must break its damn neck in a definite, but remote, time, not exceeding fifty years more.” It was a queer sensation, he felt—“this secret belief that one stands on the brink of the world’s greatest catastrophe. For it means the fall of Western Europe, as it fell in the fourth century.”⁷⁰ He began to see himself as Augustine—a failed Augustine, of course (“I aspire to be bound up with St. Augustine . . . My idea of what it should be proved beyond my powers. Only St. Augustine ever realised it”). Augustine had the advantage of the City of God. The rule of phase left room only for the City of Chaos. “A law of

⁶⁴ Lincoln, Second Inaugural Address: Lincoln to Weed, March 15, 1865, in *Works*, 8: 333.

⁶⁵ *Democratic Vistas*.

⁶⁶ “The Fortune of the Republic.”

⁶⁷ *Machiavellian Moment*, 6.

⁶⁸ Ch. 4.

⁶⁹ “The Rule of Phase Applied to History,” in Henry Adams, *The Tendency of History* (New York, 1919), 172.

⁷⁰ Henry to Brooks Adams, November 23, 1900 and May 7, 1901 in H. D. Cater, ed., *Henry Adams and His Friends: A Collection of His Unpublished Letters* (Boston, 1947), 502, 508.

acceleration," Adams wrote, "definite and constant as any law of mechanics, cannot be supposed to relax its energy to suit the convenience of man." The United States, like everything else, was finished. In the end Adams too abandoned experiment for destiny; but destiny for him was not only manifest but malign. "No one anywhere," he wrote a few weeks before the outbreak of the First World War, "... expects a future. The life is that of the fourth century, without St. Augustine."⁷¹

William James retained the experimental faith, abhorring the fatalisms and absolutes implied by "the idol of a national destiny . . . which for some inscrutable reason it has become infamous to disbelieve in or refuse." We are instructed, James said, "to be missionaries of civilization. . . . We must sow our ideals, plant our order, impose our God. The individual lives are nothing. Our duty and our destiny call, and civilization must go on. Could there be a more damning indictment of that whole bloated idol termed 'modern civilization'?" All this had come about too fast "for the older American nature not to feel the shock." One cannot know what James meant by "the older American nature"; but he plainly rejected the supposition that American motives were, by definition, pure, and that the United States enjoyed a divine immunity to temptation and corruption. Like the authors of the *Federalist*, James was a realist. "Angelic impulses and predatory lusts," he precisely wrote, "divide our heart exactly as they divide the heart of other countries."⁷²

So the warfare between realism and messianism, between experiment and destiny, continued to our own day. No recent critic of the counter-tradition was more effective than Reinhold Niebuhr with his devastating Christian polemic against the whole idea of "salvation through history."⁷³ The United States seemed to him to embody in an emphatic way the illusions of liberal culture, in great part because "we had a religious version of our national destiny which interpreted the meaning of our nationhood as God's effort to make a new beginning in the history of mankind." The Puritans had gradually shifted from emphasis on the divine favor shown to the nation to emphasis on the virtue the nation allegedly acquired through divine favor. Niebuhr defined messianism as "a corrupt expression of man's search for the ultimate within the vicissitudes and hazards of time" and warned against the "deep layer of Messianic consciousness in the mind of America." The myth of innocence was fatal to wisdom and prudence. "Nations, as individuals, who are completely innocent in their own esteem, are insufferable in their human contacts." Let the righteous nation understand the divine judgment that waits on human pretension—and not forget "the depth of evil to which individuals and communities may sink, particularly when they try to play the role of God in history."⁷⁴ So, in an ultimate irony of American history, Niebuhr used religion to refute the religious version of the national destiny.

⁷¹ Henry Adams to H. O. Taylor, November 22, 1909; to C. M. Gaskell, June 1, 1914; and to Ferris Greenslet, after December 22, 1915 in W. C. Ford, ed., *The Letters of Henry Adams, 1892-1918*, (Boston, 1938), 526, 625, 635; and *The Education of Henry Adams*, ch. 34.

⁷² Quoted in F. O. Matthiessen, *The James Family* (New York, 1961), 624-27, 631.

⁷³ Niebuhr, *Faith and History* (New York, 1949), 31.

⁷⁴ Niebuhr, *The Irony of American History* (New York, 1952), 4, 42, 69-70.

Men were corruptible, states perishable: like all other nations, America was forever on probation-time. If some political leaders were messianists, the perception of America as an experiment conducted by mortals of limited wisdom and power without divine guarantee informed the practical intelligence of others. The second Roosevelt saw life as uncertain and the national destiny problematic. The republic was still an experiment and “demands bold, persistent experimentation. It is common sense to take a method and try it: If it fails, admit it frankly and try another. But above all, try something.”⁷⁵ Franklin Delano Roosevelt’s realism kept American participation in the Second World War closer to a sense of national interest than of world mission. In a later time John F. Kennedy argued the antimessianic case: “We must face the fact that the United States is neither omnipotent nor omniscient—that we are only 6 percent of the world’s population—that we cannot impose our will upon the other 94 percent of mankind—that we cannot right every wrong or reverse each adversity—and that therefore there cannot be an American solution to every world problem.” Kennedy combined a premonition of the Machiavellian moment with an ancestral religion that understood the limits of human striving. “Before my term has ended,” he said in his first annual message, “we shall have to test anew whether a nation organized and governed such as ours can endure. The outcome is by no means certain.”⁷⁶

This evoked the mood of the Founding Fathers. But the belief in national righteousness and providential destiny remains strong—a splendid triumph of dogma over experience. One cannot but feel that this belief has encouraged our recent excesses in the world and that the republic has lost much by forgetting what James called “the older American nature.” For messianism is an illusion. No nation is sacred and unique, the United States or any other. All nations are immediate unto God. America, like every country, has interests real and fictitious, concerns generous and selfish, motives honorable and squalid. Providence has not set Americans apart from lesser breeds. We too are part of history’s seamless web.

Yet we retain one signal and extraordinary advantage over most nations—an entirely secular advantage, conferred upon us by those quite astonishing Founding Fathers. For they bequeathed us standards by which to set our course and judge our performance—and, since they were exceptional men, the standards have not been rendered obsolescent even by the second law of thermodynamics. The Declaration of Independence and the Constitution establish goals, imply commitments, and measure failures. The men who signed the Declaration, said Lincoln, “meant to set up a standard maxim for a free society which should be familiar to all, and revered by all; constantly looked to, constantly labored for, and even though not perfectly attained, constantly approximated, and thereby constantly spreading and deepening its influence and augmenting the happiness and value of life to all people of all

⁷⁵ *Public Papers*, . . . , vol. 1: 1928–1932 (New York, 1938), 646.

⁷⁶ *Public Papers* . . . 1961 (Washington, 1962), 19, 726.

colors everywhere.”⁷⁷ The values embodied in these remarkable documents constitute what Gunnar Myrdal has called the “American Creed”: “The schools teach them, the churches preach them. The courts pronounce their judicial decisions in their terms.” The conflict between creed and reality has been a powerful motive in the quest for justice. “America,” said Myrdal, “is continuously struggling for its soul.”⁷⁸

We can take pride in our nation, not as we pretend to a commission from God and a sacred destiny, but as we struggle to fulfill our deepest values in an inscrutable world. As we begin our third century, we may well be entering our golden age. But we would be ill advised to reject the apprehensions of the Founding Fathers. Indeed, a due heed to those ancient anxieties may alone save us in the future. For America remains an experiment. The outcome is by no means certain. Only at our peril can we forget the possibility that the republic will end like Gatsby in F. Scott Fitzgerald’s emblematic fable—Gatsby, who had come so long a way and whose “dream must have seemed so close that he could hardly fail to grasp it. He did not know that it was already behind him, somewhere back in that vast obscurity beyond the city, where the dark fields of the republic rolled on under the night.

“Gatsby believed in the green light, the orgastic future that year by year recedes before us. It eluded us then, but that’s no matter—to-morrow we will run faster, stretch out our arms farther. . . . And one fine morning—

“So we beat on, boats against the current, borne back ceaselessly into the past.”⁷⁹

⁷⁷ Speech at Springfield, Illinois, June 26, 1857, in *Works*, 2: 406.

⁷⁸ *An American Dilemma* (New York, 1944), 4.

⁷⁹ *The Great Gatsby*, ch. 9.



Comments:

I FOUND ARTHUR SCHLESINGER'S PAPER particularly gratifying, since it tended to confirm two of my strongest beliefs (or prejudices) about the American past. Its eloquently argued concern with the Calvinist ethos reinforced my feelings that ours is, in a serious sense, a religious society and that the facts of birthdate, sacred texts, and prophets and martyrs have given American some of the characteristics of the great missionary religions. And his conclusion, where he leaves us paddling upstream, supports my view that the United States has always, in an important sense, been a maritime civilization.

As to the meat of the argument, I am quite willing to be persuaded that the tension between the ideas of experiment and destiny, or trial and promise, has been at the core of the experience since the beginnings. The first years must have seemed very doubtful, with the City on the Hill unbuilt and the candle flickering in the wilderness. With the coming of the unwanted Revolution, the pledging of lives, liberties, and sacred honor raised real possibilities of hanging separately if it proved impossible to hang together. Yet by 1776 the promise and the destiny were also showing: Cotton Mather had had great aspirations; Hugh Brackenridge and Philip Freneau had written "The Rising Glory of America"; and Timothy Dwight, as Army chaplain, was about to compose "Columbia, Columbia, to Glory Arise." These almost millennial anticipations were, in the years after the Revolution, reinforced by the promise of the imminent real millennium, surely an event to be eagerly anticipated; but the imminence proved merely an incitement to labor more strenuously in the cause. And so it remained as the years went by: the destiny, always promised, always imminent, and always receding, repeatedly required the greatest efforts to maintain the experiment. We can see this in Abraham Lincoln's concern for the last best hope of earth, in Woodrow Wilson's call for force to the utmost to maintain the fine fabric of international law, and in John Kennedy's promise to bear any burden to assure the survival of liberty.

We must not, however, make the picture too symmetrical and unchanging. Some, at least, in the early years foresaw a diminution of the tension. Writing from Paris during the Revolution, John Adams had seen it his duty to study war and politics so that his children could study commerce and navigation, and theirs painting and porcelain. In time, in a sense, his vision was realized: a grandson married commerce and navigation in the person of a daughter of Peter Chardon Brooks; and this union produced that gloomy Henry Adams who, if he did not study porcelain, at least did study Chartres. Since Adams expected thought to reach the limit of its possibilities in 1921, we may note parenthetically that this was the year that gave us Warren G. Harding; but before we laugh too much we should remember that it was he who coined the

term “Founding Fathers,” which Schlesinger and all the rest of us now find so essential. Since Henry Adams’ time, however, we seem once again to have studied porcelain less and war and politics more. But to note this is only to note that history plays tricks on every generation of the sons of Adam, or of John Adams. The crucial paradox, pointed out by Jonathan Edwards and his disciple Samuel Hopkins, is that not everything is foreordained; one of God’s predestinating decrees is the establishment of the freedom of the will. The elect must choose to work their passage.

Despite such reservations one must say that for two hundred years or more this tension between experiment and destiny has remained strong. On a low level it permeates our lives, and particularly now as the end of the fiscal year approaches. For every good cause—your alma mater, lost animals, those in peril on the sea or in the South, children overseas—the issue is clear. Tragedy is imminent but destiny is on our side; triumph is certain, the millennium will arrive, but only if you send your check. And similarly in the larger sphere: every generation of Americans has its rendezvous with destiny. There is, one may say, floating somewhere up above us and embodying these tensions, a kind of Platonic American speech which is always relied on in time of crisis (and one can compare not only charitable appeals but the war messages from Madison down to our own day to see the similarities). To some degree, no doubt, the nature of the speech derives from the necessities of a very large and various audience, but it also derives from the past. The basic concepts are still drawn from the Awakening and the Enlightenment.

At this point two questions arise. First, in what manner have Americans historically answered the bugle? (I would say “trumpet” with Kennedy, except that it sounds a little royalist, and many of us remember bugles better than trumpets.) And second, in what kind of cause has the bugle (or the trumpet, or the still small voice) summoned us to labor so that the predestined divine decrees may be realized? The first question involves what the Calvinists used to call “means.” Here we must summon the Enlightenment to help out the Awakening, and send in Newton to support the eighteenth-century divines. Faced with great challenges, the Americans have responded greatly, and with heavy emphasis on what the Fathers called “mechanic aids”: canals and railroads, constitutions built to work like orreries, structured schemes of international government. Similarly, in warlike emergencies Newtonian mechanics appear to have governed: the pattern has been to make the mighty endeavor, to get the cart back on the road, and then with victory to demobilize instantly in the confident assumption that the laws of motion would keep it on the track.

As to the second question regarding the nature of the cause, an important part of the answer also comes from that revolutionary generation and from the mixture of patriotic aspiration with the thought of Edwards and Hopkins. This is the idea of a “disinterested benevolence” not limited by political or geographical or cultural boundaries. Writing our charitable tax-deductible checks, we are the heirs to an unparalleled tradition of good works both at

home and abroad which has provided schools and colleges, disaster relief, Bible societies and missionary efforts, and foundations large and small for more good purposes than are easy to imagine. This philanthropic tradition, unmatched by any other society, is surely one of the most American of American phenomena, so much so that in our time even the government has got itself into the act. No doubt one can find problems even in the doing of good: there is the assumption, unwelcome to some, that the philanthropist is the person with the better ideas; certainly he is the one with the money. But I take it that philanthropy is in some sense better than misanthropy; that benevolence is better than malevolence; and that—value judgments aside—it has always been very hard for Americans to pass by on the other side or to give the answer of Cain. One could, I think, construct an argument that American foreign policy since 1898 has been in large measure a succession of responses to what the missionaries used to refer to as Macedonian cries (Acts 16:9–24).

In dealing with the tension between experiment and destiny, this philanthropic apparatus can also be seen as a set of mechanic aids, to be grouped with constitutions and canals, and to be similarly employed in furthering the escape from history that began in 1776 with the escape from Europe. This, I take it, is what Henry Ford meant: history in its gloomy sense is bunk so far as the skilled mechanic is concerned. If you design a good Bill of Rights you will not be trampled down; if you run a good enough factory you can institute the \$5.00 day; if you take a scientific approach to rice breeding you can revolutionize the living standards of Asia. And one can perhaps say that, as human affairs go, these mechanical efforts have had notable success at home and some success abroad. Perhaps some measure of this achievement can be seen in the persistence of our dialogue with the Founding Fathers, at such an enormous remove in time and after such unimaginable changes in circumstance.

ONE SHOULD NOT OF COURSE SOUND TOO BICENTENNIAL. The tension remains. Some, indeed, in recent years, have been going around asking who blew out the candle in the wilderness, and even Schlesinger seems to think it has been flickering recently. Yet surely that is the nature of candles, and this one has survived some very bad times—say the winters of 1860–61, or of 1932–33, or of 1941–42. But there are still problems, and in particular one big problem which has been around a hundred years or so, and which has not quite been solved and maybe never will be. To be a little metaphorical about it, I will say that the problem is that of steam (wholly unanticipated in 1776 but visible at the first centennial in the monster Corliss engine) and the trick that steam played on an unsuspecting humanity.

Steam, one may say, is the product of coal and skills, and one of the most curious of God's predestinarian decrees was his placing of most of the coal underneath those who have had most of the skills, and pre-eminently under

the Americans. But once this decree had been revealed and steam was generated on a large scale, some very unexpected consequences developed. First, and of overriding importance, steam and its brother electricity led to power. Things could be produced at a new rate. Effort could be mobilized on a new scale. New wealth could be pooled and employed and exported, and it could also be taxed and converted into instruments of policy. All of this was wholly new to a society which had aimed fundamentally at the negation and neutralization of power, by secession from the Europe of kings, nobles, and priests and by the establishment of a government of checks and balances and prohibitions. Perhaps the single biggest problem of the second century of independence has been that of how to use this great and ever-growing power in the service of an antipower ideology.

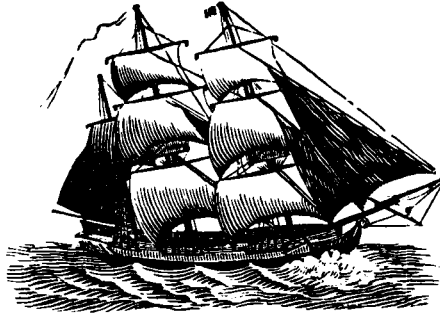
A second result of steam was the appearance of the gap. Where early American visitors to Paris and Algiers, London and St. Petersburg, Constantinople and Canton had encountered luxury of a sort undreamed of at home, by the end of the nineteenth century America was where the money was. The result, for a generation or two, was the toploftiness of the Josiah Strongs and Albert Beveridges and Woodrow Wilsons; since that time it has been an apparently ineradicable sense of guilt. A third consequence of steam and its associated phenomena, and perhaps the central problem of the third century of independence, is the crowding of the world and the advent of the age of shortages. "What a swarming crew!" wrote Melville at the end of *White-Jacket*, as he constructed his image of the world-frigate or spaceship Earth. But the crew is much bigger now, and the open-air world of codfish and beaver has given way to a world of pollution, imported strategic materials, and OPEC. Where coal once underlay the British and Americans, oil now underlies the Arabs. The God of Edwards and Hopkins seems to have apostasized and turned Turk. Experiment and destiny now struggle on a new playing field.

Still it seems there is a chance that destiny will be achieved so long as enough people work hard at the experiment. I think this was perhaps Schlesinger's conclusion as he paid a final tribute to those astonishing Founding Fathers. As we keep working on the Divine Plan we are right, I think, not to neglect their intellectual legacy. And it is interesting that Gunnar Myrdal, in his most famous work, devoted his first chapter to American ideals and the American Enlightenment, and then consciously and purposefully chose to use "enlightenment" as the final word of the book. Nor should we forget, in either their functional or their symbolic sense, the mechanic aids the Fathers left us. Here, in addition to things like the Constitution, I have particularly in mind that wonderful invention of Dr. Franklin, the bifocal lens, with which any true American can without lifting his head instantly shift his gaze back and forth between the balance sheet and the Delectable Mountains.

And finally we might remember the last sentence of the chapter of *White-Jacket* from which Mr. Schlesinger quoted: "Almost for the first time in the history of earth," wrote Melville, "national selfishness is unbounded philan-

thropy; for we cannot do a good to America but we give alms to the world.” The phrasing is perhaps archaic, reminiscent of Young America or Engine Charlie Wilson. But outside the boundaries of the United States there are many people—more than we often think—who would agree.

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ARTHUR SCHLESINGER'S PAPER IS THOROUGHLY CHARACTERISTIC of his many other writings: trenchant in argument, felicitous of phrase, facile of interpretation. It is no small accomplishment to distill and assay the national experience in twenty pages or less. Many historians have spent many more pages on the same subject and have come up with more vapor but less residual essence. The author should be congratulated as much for the attempt as for the result. And most of the following comments are made in a spirit of admiring reluctance, more to suggest limitations mostly compelled by the circumstances rather than to discredit the author's fertile intelligence and ample sense of history.

Judging from the quotations and interstitial thinking, this paper is largely theological history, Calvinist-burdened history, and New England history. In establishing his original dichotomy for early America—the Edwardian man in the hands of an angry God and the tradition of the City on the Hill—the author very rarely departs from either the intellectual climate or the geography of New England. Only in discussing the classical influence on the Founding Fathers does he go as far south as New York in one instance and Virginia in another for the tension between a sectarian Hobbesian and a reverential son of the Enlightenment.

Although admittedly our constitutional builders had a superb sense of history, I cannot see, in the major ingredients of their finished document, either the two dichotomous strains of historic Calvinism or, except in nomenclature and by way of adornment, a classical influence. In the entire corpus I think there was more of John Locke—"the philosopher," as John Adams called him—than of Calvin, much more of English history and institutions—especially the spirit of the Radical Whigs—than of Roman, and more of the nontheological, cautiously humane, compromising spirit of James Madison than of the more rigid one of Jonathan Edwards in either of

the latter's two guises. In fact, if one attempts to discuss the Bill of Rights in the then-existing theological terms, one should recognize that it reflected the mentality of the more Arminian-inclined Virginians rather than that of the New England Congregationalists and that almost its entire thrust was the protection of the multitudinous sinners in the states—the ordinary people—against the machinations of the national political elect.

Later, in talking of the more optimistic tradition, Schlesinger does mention James Polk amidst another cluster of New Englanders, some *in situ*, some transplanted (the Reverend Josiah Strong of Spanish American War days, for example). And still later, Schlesinger remarks that Christianity, turning liberal, lost its sense of original sin at about the same time that the country as a whole lost its sense of the past; into this vacuum flowed the “messianic mission,” presumably that of Woodrow Wilson at Versailles and the single-minded crusade of John Foster Dulles (“another Presbyterian,” as Schlesinger remarks) in the 1950s.

That there was much forgetting of both old memorials in the Progressive period few historians would deny. But it should be pointed out that the fundamental change—that of stressing human nurture instead of nature—inspired many internal reforms of the period and, until very recently, has been the philosophical basis for most internal liberal action. To such reforms in varied fields Schlesinger has lent—and ably so—much of his public life.

As for forgetting our history with reference to foreign affairs, certainly Woodrow Wilson has much to answer for in light of his superb training in American history, law, and political science. But exactly what was the effective mental reagent which prompted Wilson to discard 125 years of tradition to enter the European power cockpit when the nation's discernible broad national interests were not immediately imperiled? Was it a Calvinist dream of a city, more aptly a world, on a hill? Or was it—of more human stuff—the loss of face over the Sussex pledge which he had previously misconstrued to the American people? Or was it a personal pride and ambition for a historic place in the national annals? Arthur Link, after his massive and exhaustive probing into the presidential mind on this matter, asked, “Who can say with any surety of being right?”

But, if I have raised a few questions along the way, both my major commendations and my serious reservations concern Schlesinger's last few remarks on contemporary history, and his somber, if very slight, foray into the future. I am taking the liberty of also considering here another essay Schlesinger wrote right before the recent election, “The Shambles of '76,” in which he deplores the avoidance of real issues during both the Eisenhower years and the recent election campaign. I thoroughly agreed with that article when it was published and still do; election night in my opinion—in terms of issues debated and decided—offered about the same excitement as a good professional football game and had about the same significance for the nation. Certainly the spectacle of the last fifteen years is a tale spotted with the corrupt, the trivial, and the drab against a foreground of horrendous errors.

The list of major debits is so easy to construct. The Vietnam War, to which at least four administrations contributed, with its ghastly expenditure of blood and resources in an always uncertain—if not in its final chapters *perverted*—cause, says nothing about the increasing appetite of those in power for using many of the nasty and inhuman techniques of the cold war against the American people themselves. And it was precisely in this context that the republic met its most serious Machiavellian moment, when for the first time an incumbent president became utterly lawless in advancing his own personal interests, financially as well as politically. Congress also has not been of much comfort during these years—too many of its members on the take and the majority of the rest in both parties so self-interested in the perpetuation of their incumbency that they have had neither the will nor the courage in domestic and foreign affairs to meet the demanding questions which concern the very survival of the nation.

Now, I have the same doubts that the author has about either a mature man or a nation being twice born. But I am not as pessimistic as my colleague about the possibility of regeneration, about the past fifteen years of our history, or about the future. Vietnam was no Athenian Syracuse; the internal nation—despite the executive assaults upon its polity—remained whole and productive, and the threat to reasonably-decent democratic government was at least temporarily removed when its chief author was forced from office. To the great majority of American people these escapes are not *book* but *living* history, and I doubt that the strength of their examples will dissipate in the foreseeable future.

Contrary to the lament of our most recent Nobel literary laureate, a good many things worked in the sixties and the seventies. In regard to national institutions, many of our constitutional Fathers would probably have been proud of the overall record of our Supreme Court and our free press. A series of decisions by the nine old men started the nation on the way to liberating more than half the potential brains and vigor of the nation, those of women and blacks. Without the sanctuary of the first amendment, the press and television could not have substituted for Congress as a real forum for the national intelligence and national conscience. In nonpolitical matters, the sweep of Nobel prizes and that superb union of scientific and engineering brains that carried man's intelligence to Mars indicate that a good many people were doing something right in this country. Finally, I would suggest that the men of Lexington Green, Valley Forge, and Kings Mountain would have recognized some fellow spirits in that youthful, black and white, male and female, and thoroughly besieged march from Selma to Montgomery, Alabama, and again at the Washington peace mobilization, and again at Kent State University.

Not by the deity endowed, but by the luck of the historical draw and some intelligence we have been, and still are, a fortunate nation, in our peoples, in our resources, and overall in our heritage—including some very recent parts of it. If we use all three cautiously, intelligently, as the early makers of this

nation did, not to deify man to heaven nor to denigrate him to hell but to guard against his frailties and release and utilize some of his still unused potential, we have a better chance than most historic peoples have had in facing an always inscrutable and often dismaying future. But in talking about the recent past, the present, and the future every historian is entitled to be his own theologian, his own astrologer.

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Ideology and Political Culture from Jefferson to Nixon

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IN OUR OWN TIME, A REVOLUTION HAS OCCURRED in the way historians look at American politics. The scholars who shaped our discipline in the Progressive years assumed that voting and political ideology were based almost entirely on logical calculations of economic advantage. Now, our sensibilities are profoundly changed. The wars and mass hysterias through which we have made our way in the last forty years have given us a far more complex view of human nature. Entire regions of feeling and passion hitherto ignored have been opened to us. We now understand that the energies which release themselves in public life are emotional as well as rational, cultural and ideological as well as economic and pragmatic.¹

With Richard Hofstadter, we learned to explore the imagery of political rhetoric so as to discover the patterns of public ideology. He also taught us to think not only of socioeconomic class, but of specific cultural milieux in which particular moods and political world views are generated. Out of this came a new sensitivity among scholars to the power of the irrational, to tendencies among political groups to be swayed by concerns over status and by paranoid beliefs about evil conspiracies. Then, with Lee Benson and Samuel Hays, we turned our attention not just to the political elite and its ideologies, but to the

I would like to express my appreciation to the National Endowment for the Humanities for the Fellowship accorded me (1975-76) and to the Humanities Institute of the University of California for a Summer Fellowship (1976). These grants gave me the freedom to conduct much of the research on which this essay is based.

¹ By way of documentation, I shall have occasion to refer to a great many historical studies. Where pagination is not indicated, the entry of *passim* should be understood. The literature on ethnicity and its role in politics is large and can be only selectively referred to herein. The most valuable recent general study is John Higham's *Send These To Me: Jews and Other Immigrants in Urban America* (New York, 1975), especially chapter 1, "The Immigrant in American History," and the concluding chapter (11), "Another American Dilemma." Samuel Lubell, the political analyst, in his *The Future of American Politics* (New York, 1952), was perhaps the first to point to the political role of ethnicity. On the general literature of ethnocultural history, see Rudolph J. Vecoli, "European Americans: From Immigrants to Ethnic," in William H. Cartwright and Richard L. Watson, Jr., eds., *The Reinterpretation of American History and Culture* (Washington, 1973), 81-112; Robert P. Swierenga, "Ethnocultural Political Analysis: A New Approach to American Ethnic Studies," *Journal of American Studies*, 5 (1971): 59-79; Paul Kleppner, "Beyond the 'New Political History': A Review Essay," *Historical Methods Newsletter*, 6 (1972): 17-26; Samuel T. McSeveney, "Ethnic Groups, Ethnic Conflicts, and Recent Quantitative Research in American Political History," *International Migration Review*, 7 (1973): 14-33.

voters and their cultural rivalries. So informed, we now observe not simply workers opposing employers and farmers opposing bankers, as was characteristic of the class-conflict mode of analysis, but Irishmen faced off against Englishmen, Catholics against Protestants, moralists against free thinkers, Louisiana Cajun French against Anglo-Saxons, teetotalers against drinkers, and native Americans against aliens.²

It is important, of course, not to introduce a new monolithic interpretation based upon cultural considerations to replace another based on economics. The stream of national life, with its shifting crises and pressures, makes cultural issues the paramount current in one period and situation, and economic ones in another. Indeed, since voters themselves have many roles and identities, there is necessarily an intermixture of influences in public life. Cultural explanations work well with some groups, to whom identity has been a salient problem—for example, the Irish Catholics during much of American history—but with others, more subject to internal cross-pressures and less homogeneous, less certainty exists. Often, what we observe is a marginal preponderance, a shifting of the balance one way or another, so that, although most Yankees over long periods may be said to have aligned with a particular party sufficiently to give it a distinctive coloration, many were otherwise minded. Furthermore, given the nature of the available documentary evidence, relating ethnic and religious groups to voting patterns is an enterprise subject to inherent statistical risks. The volume of the evidence now, however, is very large—the patterns are linking up, and fragmentary indications in one time and circumstance gain weight as the hints they give are confirmed elsewhere—and it is possible to move with reasonable confidence in these matters. That the emerging model of politics provided by the “new political history” resembles ever more closely the politics of reality, as we observe it around us, gives the model still greater credence.

The older intellectual history of politics, in the Hofstadter style, is vulnerable in that it focused on the ideas of the leadership and was vague on the voters' identities and motives. The newer ethnocultural political history is vulnerable in that it has been unclear on how the cultural hostilities of the

² Richard Hofstadter, *Social Darwinism in American Thought* (New York, 1944), *The American Political Tradition and The Men Who Made It* (New York, 1948), and *The Age of Reform* (New York, 1955). In 1962, he wrote, “it now seems doubtful that the term ‘status politics’ . . . is an adequate term for what I had in mind. . . . If we were to speak of ‘cultural politics’ we might supply part of what is missing. In our political life there have always been certain types of cultural issues, questions of faith and morals, tone and style, freedom and coercion, which become fighting issues. To choose but one example, prohibition was an issue of this kind during the twenties and early thirties. In the struggle over prohibition, economic interests played only the most marginal role; the issue mobilized religious and moral convictions, ethnic habits and hostilities, attitudes toward health and sexuality, and other personal preoccupations.” Daniel Bell, ed., *The Radical Right: The New American Right—Expanded and Updated* (Garden City, N.Y., 1964), 99. Lee Benson's landmark study, *The Concept of Jacksonian Democracy: New York as a Test Case* (Princeton, N.J. 1961), demonstrated in depth and practically for the first time the power of ethnocultural influences in politics. Also see Samuel P. Hays, “The Politics of Reform in Municipal Government in the Progressive Era,” *Pacific Northwest Quarterly*, 55 (1964): 157–69; “The Social Analysis of American Political History, 1880–1920,” *Political Science Quarterly*, 80 (1965): 373–94, and “Political Parties and the Community-Society Continuum,” in William Nisbet Chambers and Walter Dean Burnham, eds., *The American Party Systems: Stages of Political Development* (2d ed.; New York, 1975), 152–81.

masses, usually centered in local politics, are linked to national constitutional, economic, and foreign policies. There is, however, a coherent relationship between elite ideologies and mass cultural attitudes. The ideas of the leadership and the cultural as well as economic interests of their followers do in fact align with each other, though not always easily. This congruence becomes evident if the term "cultural" is understood to refer to home-grown ethnic groups like Yankees and white Southerners, and their attitudes, as well as to those whose ethnicity is of foreign origin; if we can realize that economic rivalries often have a cultural dimension; and especially if the frame of reference is made large enough to take in the transatlantic Anglo-American community and more than just a single political generation.³

To interpret the whole of the past two centuries of American politics is a risky enterprise in the best of circumstances. It leads to a blurring of complexities, to a flattening of what on closer examination is clearly quite irregular terrain. The distillations and compressions involved create an illusion of architectonic solidity in the overall interpretation which is mocked by William James' wry commentary upon all theories: that they leak at every joint. Life is irreducibly pluralistic. There is always something left over, as he observed, which does not get included in our formulae, however carefully devised. Yet, chastened by this realization, we are still impelled to search out larger patterns. In truth, taking this long perspective upon American public life allows us to see things that we have not, I believe, seen before, and the potential gain in understanding makes the risk acceptable. In this spirit, and in the light of what I take to be the present state of historical knowledge, let us examine the course of American public life since the Revolutionary troubles began.

THE BRITISH EMPIRE BEFORE THE REVOLUTION was a single political community. England, its metropolitan component, was a nation wealthy, powerful, and sophisticated. Its people were proud and arrogant in their relations with the outlying provinces—that is, with Scotland, Wales, Ireland, and the American colonies. Englishmen looked upon Scots and Americans with amused contempt as people to be treated peremptorily. The outgroups, on their side, were bitterly conscious of these attitudes and resented them.⁴

Yet there were many in the provinces, usually but by no means always of

³ J. Morgan Kousser, in "The 'New Political History': A Methodological Critique," *Reviews in American History*, 4 (1976): 1–14, presents a sophisticated analysis of the statistical procedures of leading ethnocultural historians which leads him to express serious doubts about the validity of their results. Richard L. McCormick, "Ethno-cultural Interpretations of Nineteenth-Century American Voting Behavior," *Political Science Quarterly*, 89 (1974): 351–77, has criticized "new political historians" for not establishing links between cultural politics and national policies. George B. Tindall, in his presidential address before the Southern Historical Association, recently discussed the need to understand the Southern whites as an ethnic group; see his "Beyond the Mainstream: The Ethnic Southerners," *Journal of Southern History*, 40 (1974): 3–18, and *The Ethnic Southerners* (Baton Rouge, 1976).

⁴ Robert Kelley, *The Transatlantic Persuasion: The Liberal-Democratic Mind in the Age of Gladstone* (New York, 1969), explores ethnic relationships in Britain at length during the course of a comparative examination of the Liberal parties in Britain and Canada and the Democratic party in the United States.

English descent, who admired England and tried to make its ways of living dominant in their own regions. Anglicization, a strong cultural movement in the eighteenth-century colonies, took many forms. Its most prominent single vehicle was the Church of England. It had made itself the established church in the Southern colonies and in the four counties in and around New York City, and was pushing aggressively for more power and influence.⁵ Thus, when the troubles with England began in 1765, those who became loyalists took up the English cause because they had always cultivated an Anglicized vision of what America should become. They were nationalists too, but their nationalism was *Anglo-American*. They identified with the larger community, with the whole of the British empire.⁶

The colonies, however, were populated primarily by peoples who, in the broader British context, were indeed outsiders. Certainly this was true of the New Englanders. For a century Puritans in England had been an excluded caste. They could not vote or sit in Parliament, attend Oxford or Cambridge, or hold public office. The Anglican majority looked down upon all Dissenters—the Baptists, Quakers, and puritan Congregationalists—as strange and culturally inferior peoples whose refusal to worship in the Church of England made them not fully English.⁷ The Puritans of New England had full civil and political liberties and the satisfaction of knowing that their church was established by law; but they also knew that theirs was a specially privileged enclave.

Yankees thought of England as corrupt and fallen, except for its righteous fragment of excluded Dissenters. New Englanders were therefore ready to react angrily when England's Anglican monarch and Parliament sought to exert increased control over their lives. Furthermore, the rapidly growing trade of the British commercial empire increasingly invaded and changed their ways of living. Villages in New England, formerly quiet and stable, experienced strange new patterns of economic modernization: contractual in place of personal relations, entrepreneurism, commercial farming instead of subsistence agriculture, and the advent of luxurious living in place of simple austerities. Such things broke down the consensual communities of New

⁵ On Anglicization, see Rowland Berthoff and John M. Murrin, "Feudalism, Communalism, and the Yeoman Freeholder: The American Revolution Considered as a Social Accident," in Stephen Kurtz and James Hutson, eds., *Essays on the American Revolution* (Chapel Hill, 1973), 256–88; Michael Kraus, *The Atlantic Civilization: Eighteenth-Century Origins* (Ithaca, N.Y., 1949). Carl Bridenbaugh's *Mitre and Sceptre: Transatlantic Faiths, Ideas, Personalities, and Politics, 1689–1775* (New York, 1962) explores the question of the Church of England as do Rhys Isaac's "Religion and Authority: Problems of the Anglican Establishment in Virginia in the Era of the Great Awakening and the Parson's Cause," *William and Mary Quarterly*, 3d ser., 30 (1973): 3–36; John W. Pratt's *Religion, Politics and Diversity: The Church-State Theme in New York History* (Ithaca, N.Y., 1967); and James S. Olson's "The New York Assembly, the Politics of Religion, and the Origins of the American Revolution, 1768–1771," *Historical Magazine of the Protestant Episcopal Church*, 43 (1974): 21–28.

⁶ William H. Nelson, *The American Tory* (New York, 1961); Wallace Brown, *The King's Friends: The Composition and Motives of the American Loyalist Claimants* (Providence, R.I., 1965); Bernard Bailyn, *The Ordeal of Thomas Hutchinson* (Cambridge, Mass., 1974); Robert McCluer Calhoun, *The Loyalists in Revolutionary America, 1760–1781* (New York, 1973); Jack P. Greene, "An Uneasy Connection: An Analysis of the Preconditions of the American Revolution," in Kurtz and Hutson, *Essays on the American Revolution*, 47–49.

⁷ The literature on the social standing of Dissenters (Nonconformists) in England is extensive. In Bridenbaugh, *Mitre and Sceptre*, the eighteenth-century Anglo-American network among Dissenters is discussed, and the situation from that point forward is examined in Kelley, *The Transatlantic Persuasion*.

England. It was widely said that the English tie endangered New England's soul.⁸

Southerners, who worried about costly styles of living on the plantations, mounting debts, and the spreading disgrace of bankruptcies, resented Britain's control over tobacco prices and loan capital and regarded themselves as grievously exploited. Furthermore, the elite were a proud and prickly people who demanded personal liberty from all restraints. This attitude, widely shared by small planters as well, was so passionately held because every white person could see, in the black slaves among them, what it meant to have no liberties at all. Thus, in one of the supreme ironies of history, as Edmund Morgan has recently made clear, libertarianism fed upon slavery. Southern whites also knew that Englishmen condemned them for being slaveowners. In short, like the Yankees, in the larger context of the British empire Southern whites were also outsiders. They, too, were saying among themselves that separation from England might be necessary to fend off the moral decay of lavish living, the threat of bankruptcy, and rumored assaults upon their local autonomy.⁹

Perhaps a quarter of a million Scotch-Irish migrated to the colonies in the eighteenth century. They settled heavily in rural New York, very densely in Pennsylvania, and somewhat less extensively in the back country South. The Scotch-Irish were a stern and dour people, long accustomed to thinking that they were surrounded by enemies who must be battled continually. They brought with them a fervent Calvinist Presbyterianism and an ancient hatred of Englishmen. It was the Scotch-Irish who in the 1770s took control of the Middle Colonies and pulled them out of the empire. New York City, after all, was strongly Anglicized, Church of England, and loyal. In Philadelphia the Quaker oligarchy would have nothing to do with the patriot cause. Appalled by the Scotch-Irish Presbyterians both as Englishmen and as Quakers, they called the Scotch-Irish barbarians, whisky-drinkers, disloyal and violent men, republicans by instinct, whose ancestors had beheaded Charles I.¹⁰

⁸ Stephen E. Patterson, *Political Parties in Revolutionary Massachusetts* (Madison, 1973); Robert Zernsky, *Merchants, Farmers, and River Gods: An Essay on Eighteenth-Century American Politics* (Boston, 1971); Kenneth A. Lockridge, "Social Change and the Meaning of the American Revolution," *Journal of Social History*, 6 (1973): 403-39; Cushing Strout, *The New Heavens and New Earth: Political Religion in America* (New York, 1974), 29-55; and Alan E. Heimert, *Religion and the American Mind, from the Great Awakening to the Revolution* (Cambridge, Mass., 1966).

⁹ Rhys Isaac, "Preachers and Patriots: Popular Culture and the Revolution in Virginia," in Alfred F. Young, ed., *The American Revolution: Explorations in the History of American Radicalism* (DeKalb, Ill., 1976), 125-56, and "Evangelical Revolt: The Nature of the Baptists' Challenge to the Traditional Order in Virginia, 1765 to 1775," *William and Mary Quarterly*, 3d ser., 31 (1974): 345-68; Gordon W. Wood, *The Creation of the American Republic, 1776-1787* (Chapel Hill, 1969), 109-23; Marc Egnal and Joseph A. Ernst, "An Economic Interpretation of the American Revolution," *William and Mary Quarterly*, 3d ser., 29 (1972): 14-24; Joseph Ernst, "'Ideology' and an Economic Interpretation of the Revolution," in Young, *The American Revolution*; Edmund S. Morgan, *American Slavery American Freedom: The Ordeal of Colonial Virginia* (New York, 1975), 363-87; Merrill D. Peterson, *Thomas Jefferson and the New Nation: A Biography* (New York, 1970), 38-41; and Winthrop D. Jordan, *White over Black: American Attitudes Toward the Negro, 1550-1812* (Baltimore, 1969), 208-10.

¹⁰ Wayne L. Bockelman and Owen S. Ireland, "The Internal Revolution in Pennsylvania: An Ethnic-Religious Interpretation," *Pennsylvania History*, 4 (1974): 125-59; Patricia U. Bonomi, *A Factious People: Politics and Society in Colonial New York* (New York, 1971), 24-26; Gary B. Nash, "The Transformation of Urban

Quakers thought the term “republican” one of reproach, but to patriots it had become a proud label. For many years they had read in British newspapers a stream of violent accusations that the royal government and its wealthy supporters were trying, by a systematic use of graft and corruption, to destroy representative government in England and, therefore, liberty. These accusations were published in London by a fringe group of radical Whigs, who demanded that England purify itself by becoming a republic. That is, it should be freed from the monarchy, the established church, and the privileged aristocracy; it should be governed by an honest and austere Parliament elected by the general citizenry; and it should be rescued from the rich and mighty, who corrupted the government to serve their profiteering interests. Through the Bank of England and a monopoly on land, radicals said, capitalists and aristocrats exploited the whole community. These critics, who drew upon a long European tradition of republican political thought and looked directly to the Cromwellian Commonwealth for their inspiration, were widely believed in the colonies. Thousands were in any event predisposed, by their outgroup status within the empire, to believe the worst of the English and their government. For such persons, the existence of a Tory and Anglican conspiracy against liberty needed little proof. Few situations, indeed, have ever provided more fertile ground for the emergence of paranoid convictions about conspiratorial plots. Both sides believed in them.¹¹

As they argued with the English, patriots discovered how different were their ways of life and government from those in the home country. Soon, with Tom Paine leading the way, they insisted that America was itself essentially republican. One of the striking discoveries of recent scholarship, in fact, has been that republicanism was the distinctive political consciousness of the entire Revolutionary generation.¹² If we look closely, however, we may see that republicanism existed in several different modes, each of which contained a different center of gravity, a different national vision. New England republicans were, above everything, a pious and moralistic people. Believing in the politics of virtue, they looked to the building of a Christian Sparta, a universal Yankee nation, in the new United States. They thought of the nation at large as they did of their Puritan villages: as an organic community to be bound

Politics 1700–1765,” *JAH*, 60 (1973): 609–11; Calhoun, *The Loyalists in Revolutionary America*, 364–65; Pratt, *Religion, Politics and Diversity*, 37–77; H. James Henderson, *Party Politics in the Continental Congress* (New York, 1974), 86; John C. Miller, *Origins of the American Revolution* (Boston, 1943), 195–96; James G. Leyburn, *The Scotch-Irish: A Social History* (Chapel Hill, 1962); Alfred F. Young, *The Democratic Republicans of New York: The Origins, 1763–1797* (Chapel Hill, 1967); and James H. Hutson, *Pennsylvania Politics, 1746–1770: The Movement for Royal Government and Its Consequences* (Princeton, 1972).

¹¹ Bernard Bailyn, *The Ideological Origins of the American Revolution* (Cambridge, Mass., 1967), and *The Origins of American Politics* (New York, 1968); and Caroline Robbins, *The Eighteenth-Century Commonwealthman* (Cambridge, Mass., 1959). J. G. A. Pocock’s brilliant new study, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition* (Princeton, 1975), is now the fundamental work on republicanism as an ideology in both Europe and America and especially on its obsession with the potentialities for corruption which lie in close relations between governments and entrepreneurial capitalism.

¹² Robert E. Shalhope, “Toward a Republican Synthesis: The Emergence of an Understanding of Republicanism in American Historiography,” *William and Mary Quarterly*, 3d ser., 29 (1972): 49–80, and Eric Foner, *Tom Paine and Revolutionary America* (New York, 1976).

together in a shared way of life. The moral purity of the whole society, as an offering to God and an example to the fallen world, was their central concern. In their eyes government was a divine institution which, like the Calvinist God Himself, should be strong and active. It should guide the nation toward economic as well as moral health by direct intervention. Thus it should foster godly living and industrious habits. The American people should be self-disciplined, self-denying, hard working but not corrupted by affluence; they should be energetic, upright, and engaged in furthering God's business.

White Southerners, by contrast, were primarily *libertarian*, not moralistic, republicans. A strong government, which intervened in the lives of individuals to regulate their behavior according to moral values, was anathema. Below the Mason and Dixon line, where all but a tiny urban minority lived widely scattered in rural isolation, most envisioned a nation of white persons free to live as they saw fit. Not only was there little experience with the corporate village and town discipline so common in New England, but Southern whites were also determined that no external authority should be empowered to interfere with their freedom to keep black people in subjection. Except for its police function in support of the slave system, government should be weak and small.

The Southern gentry were often cosmopolitan in their tastes and life style, but, in their relations with London and later Washington, D.C., the planters were passionate localists. As commercial farmers, they were also hostile to bankers and merchants. Indeed, the Southern aristocracy distrusted any form of economic power, exercised from far away, over their interests. For their intellectual inspiration, many of them turned to France and Scotland, traditional enemies of the English, and acquired from those sources a critical rationalism and an anticlericalism which helped tip the balance in the South toward the separation of church and state once the Revolution had begun.

The Scotch-Irish Presbyterians of the Middle States and their allies—the Calvinists and Lutherans among the Germans, the farmers among the Calvinist Dutch, and the small but increasingly vocal urban working classes—were *egalitarian* republicans. Social rank and economic power meant English privilege and exploitation, in their experience. Some of the Scotch-Irish were aristocrats, but most were ordinary farmers who lived simple and austere lives. Scotland had always been more egalitarian and individualistic than England, with its large aristocracy and sharp consciousness of social rank. The Church of Scotland was governed in presbyterian fashion by an elected body rather than by royally appointed bishops, and it taught Scotsmen to be self-reliant, stiff-necked, and critical of the authorities.

The Quakers were right, therefore, when they accused Presbyterians of being republicans by nature. The Presbyterian-led coalition in revolutionary Pennsylvania wrote a constitution for that state which was strikingly republican. Its single-house legislature, frequent elections, and almost nonexistent executive were designed to give the people at large complete control over their government. The Scotch-Irish shared, too, that radical Whig belief that close ties between business and government inevitably led to political corruption

and social exploitation. They withdrew all government aid to Pennsylvania's manufacturers and for a time got the charter of the Bank of North America repealed.

There were thousands of people in the Middle States, however, who hated the Scotch-Irish and their egalitarian republicanism. They were predominantly English in descent, and Quaker, Anglican, or Congregationalist in religion. Most were patriots, for the taxation issue was too fundamental to avoid, but many were either reluctant rebels or outright loyalists. Centered in and around Philadelphia and New York City, they were led by an Anglicized and cosmopolitan merchant and financial elite, which had won great profits from the huge trading network of the British empire with its relatively centralized commercial structure. Now they hoped to recreate within the United States that same kind of unified economic setting. They were, in short, *nationalist* republicans. They envisioned an America centrally organized, economically activist and commercialized, orderly, and deferential to established—that is, to English—social authority. Their opportunity came after the dismal failure of the Articles of Confederation. Then they moved ahead purposively to build a strong national government.

Ironically, Virginia's leadership pushed even harder toward this goal. The vast Western territories gained in the Peace of Paris were tremendously exciting to men like George Washington and Thomas Jefferson. The settlement of that enormous region might open new markets, enabling Virginia to diversify its economy and jettison slavery, an objective attractive to many enlightened Southern aristocrats. To remove Spanish and British influence from the West and to secure title to the land from the Indians required a strong national government. Thus, a partnership emerged between the Middle States nationalists and the Virginia elite. New England acquiesced in this drive toward creation of a strong federal union, but out of fears over internal disorder spawned by Shays' Rebellion, rather than because of Western dreams. Physically closed off from the West, it was consistently to distrust that region. While the South and the Middle States poured their energies and resources into the interior, New England was forced to find its future on the high seas and in continued close ties with Old England.¹³

¹³ In arriving at this understanding of the several modes of republicanism, I have drawn upon a wide body of materials which does not lend itself to convenient documentation. Of especial aid were the perspectives developed by H. James Henderson in his superb study, *Party Politics in the Continental Congress* (New York, 1974). With regard to Massachusetts, Stephen E. Patterson's *Political Parties in Revolutionary Massachusetts* is particularly keen and insightful. Of major importance, too, are Jackson Turner Main's *Political Parties Before the Constitution* (Chapel Hill, 1973); Richard L. Bushman's *From Puritan to Yankee: Character and the Social Order in Connecticut, 1690–1765* (Cambridge, Mass., 1967); and Michael Zuckerman's *Peaceable Kingdoms: Massachusetts Towns in the Eighteenth Century* (New York, 1970). On the emerging political consciousness of the artisan and working-class urban groups, see Nash, "The Transformation of Urban Politics, 1700–65," and essays by Nash ("Social Change and the Growth of Prerevolutionary Urban Radicalism") and Dirk Hoerder ("Boston Leaders and Boston Crowds, 1765–1776") in Young, *The American Revolution*, as well as Young's *The Democratic Republicans of New York*. Gordon S. Wood's *The Creation of the American Republic, 1776–1787* is fundamental for this whole period. Also see Charles S. Sydnor, *Gentlemen Freeholders: Political Practices in Washington's Virginia* (Chapel Hill, 1952); Douglas Adair, "That Politics May be Reduced to a Science.": David Hume, James Madison, and the *Tenth Federalist*," *Huntington Library Quarterly*, 20 (1957): 343–60; Kelley, *The Transatlantic Persuasion*, 101–44; Bockelman and Ireland, "The Internal Revolution in

UNDER THE NEW FEDERAL GOVERNMENT, the nationalist republicans of the Middle States and the moralistic republicans of New England soon joined forces to form the Federalist party and govern the country. Their essential interests and their visions of what the United States should become flowed together, though not entirely comfortably, as the bitter rivalry between Alexander Hamilton and John Adams tells us. With support from former loyalists, the Federalists set out to build a strong and vigorous nation modeled on English lines. It was to be Anglophilic in sentiment, Anglicized in political style, Atlantic and eastward-looking in orientation, and commercial in economic policy. Thus, its enemy was to be England's: that is, France, and that nation's radicalism, irreligion, and egalitarianism. At home, the United States was to have an economy given central direction by financial institutions patterned after the Bank of England. As a political outlook which flourished primarily among those of English descent, Federalism had its deepest roots in New England and among the Anglicized aristocracy of the Middle States. Elitist in social values, moralistic, and aggressively Protestant, the Federalists regarded the egalitarian Scotch-Irish and Germans of all faiths with contempt.

The South, however, soon took national leadership away from the Federalists, for it was moving with the national tide. Its Western orientation, hostility to the English, and white supremacist libertarianism found strong support among the ethnic minorities in the Middle States. Furthermore, the Southern distrust of bankers and of the remote economic power they represented was widely shared in the country at large. So, too, was its localism, for the Revolution was over, the constitutional crisis was resolved, and nationalism was fading. In any event, south of New England there was a distaste for Yankees. The ethnic minorities resented the New Englanders' belief in their superior moral righteousness, their assumption that—as host culture—they were the true Americans, and their urge to reshape the nation in the Yankee image. The minorities well knew, furthermore, that Federalists considered the ethnic groups undesirable and potentially disloyal people whose presence in the United States endangered the cultural hegemony of those of English descent.¹⁴

The Jeffersonian Republican party took form in the mid-1790s and inaugurated a two-party rivalry which was not to subside until the 1820s. The core of Jefferson's party was a coalition combining libertarian white Southerners

Pennsylvania"; Hutson, *Pennsylvania Politics, 1746-1770*; Merrill Jensen, *The American Revolution within America* (New York, 1974); and William McLoughlin, *New England Dissent, 1630-1833: The Baptists and the Separation of Church and State* (Cambridge, Mass., 1971).

¹⁴ John C. Miller, *Alexander Hamilton and the Growth of the New Nation* (New York, 1964); Jerald A. Combs, *The Jay Treaty: Political Battleground of the Founding Fathers* (Berkeley, 1970); Alexander De Conde, *The Quasi-War: The Politics and Diplomacy of the Undeclared War with France, 1797-1801* (New York, 1966); Gerald Stourzh, *Alexander Hamilton and the Idea of Republican Government* (Stanford, 1970); Rudolph M. Bell, *Party and Faction in American Politics: The House of Representatives, 1789-1801* (Westport, Conn., 1973); Richard Buel, Jr., *Securing the Revolution: Ideology in American Politics, 1789-1815* (Ithaca, N.Y., 1972); James Banner, *To the Hartford Convention: The Federalists and the Origins of Party Politics in Massachusetts, 1789-1815* (New York, 1970); and Linda K. Kerber, *Federalists in Dissent: Imagery and Ideology in Jeffersonian America* (Ithaca, N.Y., 1970).

with the egalitarian ethnic minorities, urban artisans, and working people of the Middle States. By 1800 the Republicans were strong enough to sweep Thomas Jefferson into the White House and to take control of Congress. For most of the next sixty years the South and the Middle Atlantic States together were able to maintain effective control of the national government. Under Thomas Jefferson, the Republicans set out to turn the country back to what they believed were more traditional, more genuinely American—as opposed to Anglicized—directions. Each state and local region was largely to govern itself; farming, not commerce, was to provide the nation's livelihood.¹⁵

The crucial issue between the parties, however, remained that most ancient one: America's relationship with England. Federalists wanted the United States to support that country in its fight against Napoleonic France, but Jeffersonians, inveterate Anglophobes, insisted upon American rights of neutrality on the high seas. In time, American trade and self-respect were so throttled and endangered by the British fleet that, to Jeffersonians, the republic itself seemed in peril. The result was the second English war, fought by Jeffersonian Republicans and opposed by New England Federalists to the bitter end. When the Scotch-Irishman Andrew Jackson won his spectacular victory over the British before New Orleans in 1815, the republic and republicanism finally seemed safe.¹⁶

Meanwhile, Jeffersonian Republicans pre-empted the nationalist position. Forced by the long crisis to think of national power and coherence, they created the Second Bank of the United States, a modestly protective tariff against British goods, and a program by which, through the building of the National Road, the interior and its resources would be opened. Federalism, furthermore, was widely condemned as disloyal for its role during the war, and in the 1820s it slowly dissolved as a party. With their enemy gone, Jeffersonian Republicans broke into feuding fragments. The first, or Jeffersonian party system, came to an end.¹⁷

It was soon essentially revived, however, around the person of Andrew

¹⁵ Bell, *Party and Faction in American Politics*; Buel, *Securing the Revolution*; Noble E. Cunningham, Jr., *The Jeffersonian Republicans: The Formation of Party Organization, 1789-1801* (Chapel Hill, 1957); Peterson, *Thomas Jefferson and the New Nation*; Harry Marlin Tinkcom, *The Republicans and Federalists in Pennsylvania, 1790-1801: A Study in National Stimulus and Local Response* (Harrisburg, 1950); Eugene Perry Link, *Democratic-Republican Societies, 1790-1800* (New York, 1965); Young, *The Democratic Republicans of New York*; David Hackett Fischer, *The Revolution of American Conservatism: The Federalist Party in the Era of Jeffersonian Democracy* (New York, 1965); Alfred F. Young, "The Mechanics and the Jeffersonians: New York, 1789-1801," *Labor History*, 5 (1964): 247-76; Morton Borden, *Parties and Politics in the Early Republic, 1789-1815* (New York, 1967), and *The Federalism of James A. Bayard* (New York, 1955); and Paul Goodman, *The Democratic-Republicans of Massachusetts: Politics in a Young Republic* (Cambridge, Mass., 1964).

¹⁶ Robert H. Brown, *The Republic in Peril, 1812* (New York, 1964); Banner, *To the Hartford Convention*; and Fischer, *The Revolution of American Conservatism*.

¹⁷ Shaw Livermore, Jr., *The Twilight of Federalism: The Disintegration of the Federalist Party, 1815-1830* (Princeton, 1962); Paul Goodman, "The First American Party System," in Chambers and Burnham, *The American Party Systems*, 56-89; and Harry Ammon, *James Monroe: The Quest for National Identity* (New York, 1971). The book which lays out, on the basis of quantitative studies, the concept and chronology of the five party systems is Walter Dean Burnham's *Critical Elections and the Mainsprings of American Politics* (New York, 1970). Ronald P. Formisano has recently maintained that a party system did not really take form until 1840, raising strong objections to the concept of the "first party system"—referring to the Jeffersonian period—now in general usage. See his "Deferential-Participant Politics: The Early Republic's Political Culture, 1789-1840," *American Political Science Review*, 68 (1974): 473-87.

Jackson. In the second, or Jacksonian party system, Democrats lined up behind Jackson and Whigs opposed him. The South and the Middle Atlantic States again dominated the federal government. The Western states voted Jacksonian when he took the presidency in 1828, but a huge migration from New England soon made the West a political battleground between transplanted Yankees and the Southerners who had settled there earlier. A branch of Whiggism, furthermore, flourished in Kentucky and Tennessee. Thus there emerged a two-party rivalry which was fully national—in that it flourished in practically every state—and was remarkably evenly balanced. By 1840 the parties were so meticulously organized down into every ward and precinct and were so successful in mustering the citizenry in huge numbers to the polls that we may finally speak of the full emergence, in modern terms, of mass political parties, the first in the world.¹⁸

The great issue that obsessed the Jacksonians was the sweeping economic revolution of these years. Whigs looked upon that revolution with cheer, praising growth and industrialization. Democrats grieved over the loss of a simpler past; they talked constantly of the need for governments to cease handing out special privileges to particular businessmen, and to stand back and let free competition between equal individuals insure social justice. Jackson attacked the Second Bank of the United States as a machine of capitalist and English oppression; Henry Clay battled for his economically activist American system, which called upon the government to stimulate the economy by establishing protective tariffs, internal improvements, and a national bank. Democrats insisted that political corruption was the inevitable outcome of Whig policies. In state legislatures Whigs urged economic development and praised banks and corporations; Democrats distrusted them and tried to limit their freedom of action by antibank legislation and regulation of corporate business practices.¹⁹

A natural partnership existed between those who attacked capitalist power and those who were anti-Yankee, for they shared the same enemy. Employers

¹⁸ Richard P. McCormick, *The Second American Party System: Party Formation in the Jacksonian Era* (Chapel Hill, 1966); Formisano, "Deferential-Participant Politics," and *The Birth of Mass Political Parties: Michigan, 1827-1861* (Princeton, 1971); Joel Silbey, *The Shrine of Party: Congressional Voting Behavior, 1841-1852* (Pittsburgh, 1967); T. B. Alexander, *Sectional Stress and Party Strength: A Study of Roll Call Voting Patterns in the United States House of Representatives, 1836-1860* (Nashville, Tenn., 1967); and Richard H. Brown, "The Missouri Crisis, Slavery, and the Politics of Jacksonianism," *South Atlantic Quarterly*, 65 (1966): 55-72. The power of the Democratic party in Pennsylvania is made clear in Philip S. Klein and Ari Hoogenboom, *A History of Pennsylvania* (New York, 1973), 118-24; Charles McC. Snyder, *The Jacksonian Heritage: Pennsylvania Politics, 1833-1848* (Harrisburg, 1958); John M. Belohlavek, "The Democracy in a Dilemma: George M. Dallas, Pennsylvania, and the Election of 1844," *Pennsylvania History*, 41 (1974): 391-411; and Bruce I. Ambacher, "The Pennsylvania Origins of Popular Sovereignty," *Pennsylvania Magazine of History and Biography*, 98 (1974): 339-52, and "George M. Dallas and the Bank War," *Pennsylvania History*, 42 (1975), 117-35. I have explored Jacksonian politics in New York state in *The Transatlantic Persuasion*, 238-265.

¹⁹ Arthur M. Schlesinger, jr., *The Age of Jackson* (Boston, 1945); Marvin Meyers, *The Jacksonian Persuasion: Politics and Belief* (Stanford, 1957); Kelley, *The Transatlantic Persuasion*, 238-65; Glyndon G. Van Deusen, "Some Aspects of Whig Thought and Theory in the Jacksonian Period," *AHR*, 63 (1958): 305-22, *The Jacksonian Era, 1828-1848* (New York, 1959), and "The Whig Party," in Arthur M. Schlesinger, jr., ed., *History of U.S. Political Parties* (New York, 1973), 1: 333-63; and Herbert Ershkowitz and William G. Shade, "Consensus or Conflict? Political Behavior in the State Legislatures During the Jacksonian Era," *JAH*, 58 (1971): 591-621.

and bankers were usually English or at least British in their ethnic origins; workers were often Irish or German. The wealthy were overwhelmingly English and of the socially prestigious Protestant sects. In Illinois, divided between a Yankee north and a Southern south, the Yankees urged economic progress and supported banks, while the Southerners and pockets of German farmers resisted. It made cultural sense, therefore, for the Jacksonian Democrats endlessly to echo Thomas Jefferson's assertion that there is an inevitable and never-ending combat between the wealthy and the powerful, who always seek special advantages to their own profit, and the rest of society.²⁰

The cultural confrontation between Whigs and Democrats ran across the entire spectrum of national life. Andrew Jackson, the son of Scotch-Irish immigrants, received the classic epithets which Englishmen traditionally threw at Scotch-Irishmen: that he was a violent barbarian, a drunkard, and a crude immoral person.²¹ These were in fact violent years. Mobbings and urban disorders mounted alarmingly. The Whigs, calling themselves the party of law and order, consistently blamed all violence on the ethnic minorities. They tried hard to deny votes to immigrants, demanding, in the words of the leader of the Whigs in Michigan, that voters be "of this Anglo-American family . . . [their] *habits* must . . . have been formed upon our model." Whig political picnics were like evangelical church-camp meetings, drawing powerful support from the aggressively pious and moralistic. In order to reform the aliens, they campaigned regularly for temperance and Sabbath laws.²²

The Democrats, on their side, called Whigs church-and-state fanatics, welcomed non-English immigrants, and demanded personal liberty in moral behavior. When Yankee ministers attacked drinking and Sabbath amuse-

²⁰ The ethnic component of class rivalries has not yet been explored with nearly as much care as that which has been done in politics. Labor history still continues to operate broadly within the older categories of economic class, as Gerald Rosenblum has recently remarked in his *Immigrant Workers: Their Impact on American Labor Radicalism* (New York, 1973), 26. Frank Otto Gatell has made strikingly clear how British, and how Congregational, Presbyterian, or Episcopalian were the wealthy of New York City in his "Money and Party in Jacksonian America: A Quantitative Look at New York City's Men of Quality," *Political Science Quarterly*, 82 (1967): 235-52. Lee Benson's *The Concept of Jacksonian Democracy* explores the interpenetration of ethnicity and economic class with striking sensitivity and detail. William G. Shade, in *Banks or No Banks: The Money Question in the Western States, 1832-1865* (Detroit, 1973), and James Roger Sharp, in *The Jacksonians versus the Banks: Politics in the States after the Panic of 1837* (New York, 1970), both delineate with keen perception the cultural differences on the bank issue. See also John M. McFaul, *The Politics of Jacksonian Finance* (Ithaca, N.Y., 1972); and see Merrill D. Peterson's brilliant study, *The Jefferson Image in the American Mind* (New York, 1962), for Jefferson's ideology of social conflict.

²¹ It is striking how little attention modern biographers of Andrew Jackson pay to his ethnic identity. His first biographer, James Parton, however, is revealingly full and elaborate on this point. An Englishman himself, he concluded his superb *Life of Andrew Jackson* (New York, 1860) with a harsh personal attack upon Jackson, written from the vantage point of one who had lived through much of the Jacksonian era. But he began with a lengthy commentary upon the cultural traits of Ulster Irishmen. Jackson was born soon after his parents arrived in America from Ulster and spoke with an Irish burr. The Scotch-Irishman was still then part of an ethnic community which stood out sharply from Englishmen.

²² David Grimsted, "Rioting in its Jacksonian Setting," *AHR*, 77 (1973): 361-97; Formisano, *Birth of Mass Political Parties*, 104-49; Ershkowitz and Shade, "Political Behavior in the State Legislatures During the Jacksonian Era"; Bertram Wyatt-Brown, "Prelude to Abolitionism: Sabbatarian Politics and the Rise of the Second Party System," *JAH*, 58 (1971): 316-41; and Benson, *The Concept of Jacksonian Democracy*. On this general topic of the interpenetration of the Protestant religion and American politics, also see William G. McLoughlin, "The Role of Religion in the Revolution: Liberty of Conscience and Cultural Cohesion in the New Nation," in Kurtz and Hutson, *Essays on the American Revolution*, 206-08.

ments, the Scotch-Irish with their ethnic beverage (whisky) and the Germans with their Continental Sunday and their beer joined Southern whites in insisting upon libertarianism. Even the Baptists and Methodists agreed in the Jacksonian years, for both resented the aggressive and socially dominant Calvinists. Roman Catholics were close political allies of the anticlericals and free-thinkers on the Jacksonian side, for only a strict separation between church and state would keep the Puritans from imposing their ways on Catholics. In Pennsylvania, these same issues even pitted Germans against Germans. The Lutherans were an inward turning people who wanted to be left alone, and they voted solidly Democratic; the Moravians were by contrast a zealous and morally activist folk much like the Puritans, and they praised Yankee schools and Yankee inventiveness and voted strongly Whig.²³

A striking reversal in these alignments occurred in the South. In that region the Jacksonian Democrats, not the Yankees, comprised the host culture and were identified with everything archetypically Southern: slaveholding, a single crop economy, and a widely dispersed, individualistic, and almost frontier existence. Thus, to be a Whig in the South was in a certain sense to declare oneself an outsider. The French Catholic Cajuns of Louisiana, who detested the Jacksonian Democrats pushing in on them, were Whig. Those living in Appalachian areas relatively free of slaves, who hated the system and the arrogant slaveholders it produced, were strongly Whig. Those generally urban peoples who admired Yankee ways wanted to see the South become more Northern. Like the loyalists during the Revolution, they identified with the larger national community. Merchants, bankers, professional men, and, indeed, many wealthy plantation owners who voted Whig moved against the current of Southern life: against its violence, its persistent crudity in living conditions, its touchy egalitarianism, and its obsessive hatred of Yankees. Such Southerners wanted a diversified economy, brisk and tidy cities, and a more modernized life-style. They represented, in short, what Carl Degler has aptly termed the "other South." Together these peoples in the Whig coalition actually won a slight majority of popular votes cast in Southern elections in the years from 1836 to 1848.²⁴

²³ Benson, *The Concept of Jacksonian Democracy*; Formisano, *Birth of Mass Political Parties*; Lois W. Banner, "Religious Benevolence as Social Control: A Critique of an Interpretation," *JAH*, 60 (1973): 23-41; Whitney R. Cross, *The Burned-Over District: The Social and Intellectual History of Enthusiastic Religion in Western New York, 1800-1850* (Ithaca, N.Y., 1950), 46-47, 131; Robert W. Doherty, "Social Bases for the Presbyterian Schism of 1837-1838: The Philadelphia Case," *Journal of Social History*, 2 (1968): 69-79; Thomas T. McAvoy, "The Formation of the Catholic Minority in the United States—1820-1860," *Review of Politics*, 10 (1948): 13-34; Edward M. Levine, *The Irish and Irish Politicians: A Study of Cultural and Social Alienation* (London, 1966), 53-75; William G. Shade, "Pennsylvania Politics in the Jacksonian Period, a Case Study: Northampton County, 1824-1844," *Pennsylvania History*, 39 (1972): 313-33; and Vincent P. Lannin, "Alienation in America: The Immigrant Catholic and Public Education in Pre-Civil War America," *Review of Politics*, 32 (1970): 503-21.

²⁴ I am heavily indebted to Carl N. Degler's pathfinding study, *The Other South: Southern Dissenters in the Nineteenth Century* (New York, 1974). There was also an "other South" in the Southern Federalists: Lisle A. Rose, *Prologue to Democracy: The Federalists in the South, 1789-1800* (Lexington, Ky., 1968); and James H. Broussard, "Regional Pride and Republican Politics: The Fatal Weakness of Southern Federalism, 1800-1815," *The South Atlantic Quarterly*, 73 (1974), 23-33. It was similarly Northern-oriented in opposition to the broader trends in Southern life. For the Jacksonian period, see John Vollmer Mering, *The Whig Party in*

THE ISSUE OF THE EXPANSION OF SLAVERY destroyed the second party system. After it first erupted, with the Mexican War and the Wilmot Proviso in 1846, the Jacksonian party system struggled with the crisis for some years with reasonable success.²⁵ In 1854, however, the Kansas-Nebraska Act ripped everything wide apart. The Whigs were destroyed as a party because they could no longer straddle the question; the South swung back massively to one-party dominance under the Democrats; a new Northern party, the Republicans, came into being; and the third, or Civil War, party system was born.

Much more than the slavery issue, however, brought this about. A huge influx of Irish Catholics in the 1840s set off an explosive anti-Catholic and anti-alien crisis in the 1850s. The American or Know-Nothing party emerged to attack the Catholics. It pulled out of the older parties thousands of Protestant voters, many of them Methodists and Baptists in the Northern states who had long voted Democratic on the basis of personal liberty. When the Know-Nothings in their turn were destroyed by the slavery issue, their Northern voters moved en bloc into the new Republican party. Another influence which stimulated these movements was a rekindling of temperance and Sabbatarian crusades in the 1850s. These issues drew even more thousands into Republican ranks, for the core of that party lay among the Yankees and their moralism.²⁶

Republicans were proud of Northern civilization. They saw it, in classic Yankee terms, as vigorous, hard-working, truly religious, and forward-looking. Southern white civilization, by contrast, seemed to them stagnant and slothful, degraded by its distaste for manual labor, and made immoral, retrograde, and un-Christian by slavery. What Republicans now feared was that Southern conspirators, aided by Democratic allies in the North, had gotten control of the national government and intended to make slavery nationwide. In this sense, the national argument over the expansion of slavery was fundamentally a conflict of cultures. It was a struggle, as Republicans saw it, to settle the question of whether the United States, both South and North, would become essentially Southern or Northern. On their side, white Southerners became convinced that the whole of the North was being taken over by a conspiracy of abolitionists. Many were terrified that such an abolitionist

Missouri (Columbia, Mo., 1967); Charles G. Sellers, Jr., "Who Were the Southern Whigs?" *AHR*, 59 (1954): 335-46; McCormick, *The Second American Party System*, 310-20; Burton W. Folsom II, "The Politics of Elites: Prominence and Party in Davidson County, Tennessee, 1835-1861," *Journal of Southern History*, 39 (1973), 359-79; William R. Taylor, *Cavalier and Yankee: The Old South and American National Character* (Garden City, N.Y., 1963), 138, 189; and Silbey, *The Shrine of Party: Congressional Voting Behavior, 1841-1852*.

²⁵ Chaplain W. Morrison, *Democratic Politics and Sectionalism: The Wilmot Proviso Controversy* (Chapel Hill, 1967); and John H. Schroeder, *Mr. Polk's War: American Opposition and Dissent, 1846-1848* (Madison, 1973). Joseph G. Rayback, in *Free Soil: The Election of 1848* (Lexington, Ky., 1970), observes, "the strength of [party loyalty] . . . in the presidential election of 1848 was the largest single factor contributing to the results. In an election in which divisive forces were very powerful, the overwhelming majority of both parties remained loyal to party" (288). See also Silbey, *The Shrine of Party*, 143-44.

²⁶ Michael F. Holt, "The Politics of Impatience: The Origins of Know-Nothingism," *JAH*, 60 (1973): 309-31, and *Forging a Majority: The Formation of the Republican Party in Pittsburgh, 1848-1860* (New Haven, 1969), 133-51; Eric Foner, *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970), 237-60; Formisano, *Birth of Mass Political Parties*, 205-38; and David Potter, *The Impending Crisis, 1848-1861* (New York, 1976), 225-66.

victory would lead to a bloodbath in the South, with widespread murder and rapine by freed slaves.²⁷

The lower tier of Northern states played the crucial role in this controversy. Lying roughly below the 41st parallel, this band of territory included the great port cities of New York and Philadelphia and the southern parts of Pennsylvania, Ohio, Indiana, and Illinois. The larger cities were ethnically mixed; much of the countryside was Scotch-Irish, Dutch, and German; and the Middle-Western segment had been settled primarily by Southerners. The lower North had traditionally leaned southward: it distrusted Yankees and violently condemned abolitionist talk. Now, however, the arrival of Catholic Irish produced a landmark shift: the Scotch-Irish joined the English, their ancient enemies, in the Republican party out of an even greater hatred of the Catholic Irish in the Democratic party.²⁸

Also, a new consciousness had emerged in the lower North and formed around a new national leader, Abraham Lincoln. Revealingly, Lincoln was a person of Southern origins who turned away from Southern values. Born in Kentucky, he had grown up in that pervasively Southern world which dominated southern Indiana and Illinois. Jacksonian Democracy flourished there, but Lincoln veered off, becoming a Whig and a firm disciple of Henry Clay. Like the Yankees, he believed in encouraging economic development. "My politics," he said in his first political speech in 1832, "are short and sweet . . . I am in favour of the internal improvement system and a high protective tariff."

His Whiggism was much more, however, than just an economic attitude. In a hard-drinking frontier society, he avoided alcohol and counseled temperance. Surrounded by cigars and spittoons, he did not smoke or chew. In a violent society obsessed with guns, he would not even use them to hunt. Believing that only those who paid taxes should vote, he opposed universal manhood suffrage. In an aggressively male society, he advocated votes for women. Abraham Lincoln was a Whig, one must conclude, because he preferred what Whigs believed to be a more civilized way of living. Furthermore, he was genuinely offended by the institution of slavery. In his own ambivalent way, he wanted to make a better life possible for black Americans, even if he could not conceive for them full social and political equality. Northern Democrats, on the other hand, ranted white supremacist doctrines and often approved of slavery. The Irish Catholics, to take an extreme example, set off in New York City during the Civil War the most violent race

²⁷ Foner, *Free Soil, Free Labor, Free Men*, 1-72; and David Brion Davis, *The Slave Power Conspiracy and the Paranoid Style* (Baton Rouge, 1969), which also describes the Southern fears of an abolitionist conspiracy in the North. Steven A. Channing, *Crisis of Fear: Secession in South Carolina* (New York, 1970), is especially persuasive on fears of slave rebellion.

²⁸ Foner, *Free Soil, Free Labor, Free Men*, 186-216; and James A. Rawley, *Race and Politics: Bleeding Kansas and the Coming of the Civil War* (Philadelphia, 1969). For the Scotch-Irish we do not yet have quantitative evidence as to their movement into the Republican party during this period, and I am going on impressionistic data at this point. The hatred of the Protestant Irish (Scotch-Irish) for the Catholic Irish is, of course, legendary, and Rowland Tappan Berthoff, in his *British Immigrants in Industrial America, 1790-1950* (Cambridge, Mass., 1953), speaks at length of the discovery of their common "Britishness" by the Welsh, English, and Scots after the arrival in this country of the Catholic Irish, against whom in bitter hostility they ranged politically.

riot of the nineteenth century rather than obey the draft and fight against slavery.²⁹

Lincoln insisted that, far from allowing the spread of slavery, the national government should treat it with the greatest possible constitutional unfriendliness in the hope that it would die of its own accursed weight. When the lower North agreed, leaving its traditional moorings in the election of 1860 to join with the upper North against the slave states, the national balance of power shifted finally and fatefully northward, where it thereafter remained. The Deep South, in the best of circumstances a region whose white citizens were distrustful and contemptuous toward Yankees, and now led by extreme Southern nationalists, left the Union rather than face the unspeakable: a violent slave uprising which, most whites had convinced themselves, a Republican victory would set in motion. They saw no alternative to secession. Abraham Lincoln, the ultimate republican (in the Revolutionary sense) and American nationalist, could not on his side accept that the world's "last, best hope" should collapse, and he took the North to war in its cause. The upper South, forced by Lincoln's call for troops to choose, reluctantly followed the Deep South into the Confederacy (save for the border states); and the war came.

As the conflict raged, fundamental shifts occurred in the North. The Republicans finally were able to establish that nationalist economic system for which the Whigs had so long called: protective tariffs, a national banking and currency system, and the spending of huge sums on internal improvements to open up the country's resources. Southern reconstruction, carried through in a triumphant mood of Yankee supremacy, aimed not only at establishing legal and political equality for Southern blacks, but at revolutionizing Southern white society in the Northern image as well. To the delight of Southern businessmen and former Whigs, the creation of a vigorously commercial civilization in the South was encouraged. The "other South" had been strong in the Confederate Congress during the war; now its leaders took heart and sought under various political names to distinguish themselves from the Democrats in local politics.³⁰

²⁹ In this discussion of Lincoln, I have been most aided by George M. Fredrickson, "A Man but Not a Brother: Abraham Lincoln and Racial Equality," *Journal of Southern History*, 41 (1975): 39-58; Don E. Fehrenbacher, *Prelude to Greatness: Lincoln in the 1850s* (New York, 1964); the classic study of David Donald, *Lincoln Reconsidered* (New York, 1956); Lord Charnwood, *Abraham Lincoln* (New York, 1917); Richard Hofstadter, "Abraham Lincoln and the Self-Made Myth," in his *The American Political Tradition and the Men Who Made It* (New York, 1949), 92-134; Paul M. Angle, ed., *Created Equal? The Complete Lincoln-Douglas Debates of 1858* (Chicago, 1958); and Ronald D. Rietveld, "Lincoln and the Politics of Morality," *Illinois State Historical Society Journal*, 68 (1975): 27-43. For the Republican position in general toward slavery and black Americans, see Foner, *Free Soil, Free Labor, Free Men*, 261-300. For the attitudes of Democrats and the North in general toward race issues, see Robert W. Johannsen, *Stephen A. Douglas* (New York, 1973), 233, 340, 501; Gilbert Osofsky, "Abolitionists, Irish Immigrants, and the Dilemmas of Romantic Nationalism," *AHR*, 80 (1975): 889-912; Leon F. Litwack, *North of Slavery: The Negro in the Free States, 1790-1860* (Chicago, 1961); George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817-1914* (New York, 1971); and V. Jacques Voegeli, *Free but Not Equal: The Midwest and the Negro During the Civil War* (Chicago, 1967).

³⁰ Foner, *Free Soil, Free Labor, Free Men*, 181-85; Leonard P. Curry, *Blueprint for Modern America: Non-Military Legislation of the First Civil War Congress* (Nashville, 1968), 244-49; and George M. Fredrickson, *The*

When Southern Democrats were able to re-enter national politics in the 1870s, they produced so even a balance in Washington between the two major parties that twenty years of stalemate ensued. Democrats in the North were flourishing. Many voters disliked the Republicans' pro-black policies; nationalism was fading as the war receded; and a political corruption which Democrats had always insisted would flow naturally from Republican policies of aid to business enterprise seemed to infect government at every level. Renewed Irish and German immigration also brought thousands of new voters into the party. When in the 1880s Yankee Republicans resumed their fight for prohibitionism, condemned parochial Catholic and Lutheran schools, and participated in a nativist outburst which sought to close off immigration and deny political equality to aliens, the immigrants became passionate Democratic activists.

The politics of the Gilded Age were not, in reality, the shallow and meaningless politics of historical myth. The people of the Gilded Age battled about economic and cultural issues which strikingly resemble those of the Jacksonian era. They built the most intricately organized grass-roots politics in American history, turning out at the polls in enormous numbers, in proportions not yet to be matched. Grover Cleveland, reared a Jacksonian Democrat and an old-style Presbyterian of Scottish theology, regularly issued jeremiads on the sordidness and selfishness which, he believed, Republicans had introduced into national life, and on corporate greed and the grinding down of the poor. He and his mugwump following in the educated Northeastern elite admired the British Liberals, from whom they drew an almost religious commitment to civil service reform to end corruption and to lowered tariffs to wipe out economic privilege and create social justice. In 1892 Cleveland was carried to the White House for his second term in a massive victory which saw the Democrats also take control of Congress for the first time since before the Civil War.³¹

Inner Civil War: Northern Intellectuals and the Crisis of the Union (New York, 1965). David Donald's *Charles Sumner and the Rights of Man* (New York, 1970) as well as his many other studies of the Civil War and Reconstruction are essential in capturing the Yankee mind in this period. Willie Lee Rose's sensitive exploration, *Rehearsal for Reconstruction: The Port Royal Experiment* (New York, 1967), shows the Yankee mood at work, from its high expectations at the outset to its final defeat. "Gone were the grand claims," she writes, "of making a new New England of the South" as Reconstruction faded (page 388). Also see C. Vann Woodward, *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1971), for Yankee attitudes. On the reaction of the "other South," see Degler, *The Other South*, 124-266; Thomas B. Alexander, "Persistent Whiggery in the Confederate South, 1860-1877," *Journal of Southern History*, 27 (1961): 205-29; T. B. Alexander and Richard E. Beringer, *The Anatomy of the Confederate Congress: A Study of the Influences of Member Characteristics on Legislative Voting Behavior, 1861-1865* (Nashville, Tenn., 1972); Jack P. Maddex, Jr., *The Virginia Conservatives, 1867-1879: A Study in Reconstruction Politics* (Chapel Hill, 1970); Otto H. Olsen, "Reconsidering the Scalawags," *Civil War History*, 12 (1966): 304-20; and Paul M. Gaston, *The New South Creed: A Study in Southern Mythmaking* (New York, 1970).

³¹ This general picture is now emerging in a rush of new historical studies on the Gilded Age. See Geoffrey Blodgett, "A New Look at the American Gilded Age," *Historical Reflections*, 1 (1974): 232, 237, and *The Gentle Reformers: Massachusetts Democrats in the Cleveland Era* (Cambridge, Mass., 1966); Richard Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888-1896* (Chicago, 1971); Kelley, *The Transatlantic Persuasion*, 276-350; Gerald W. McFarland, *Mugwumps, Morals and Politics, 1884-1920* (Amherst, Mass., 1975); Ari Hoogenboom, *Outlawing the Spoils: A History of the Civil Service Reform Movement 1865-1883* (Urbana, 1961); John Higham, *Strangers in the Land: Patterns of American Nativism, 1860-1925* (New York, 1963); Tom E.

NOW, HOWEVER, AN ECONOMIC CRISIS of earthquake proportions swept in to transform national politics. The great depression of the 1890s destroyed Cleveland's hopes, his Democratic coalition, and the third party system. Urban peoples suffered as never before, and they were joined in their distress by Southern and Western farmers. The story of Populism and its explosive growth in the Great Plains and the South, its demand for sweeping national reforms and above all for currency inflation through the free coinage of silver, has long been familiar. Through William Jennings Bryan the Populist creed now captured the Democratic party and transformed it into a Southern and Western party. New England had always distrusted both the South and the West; inflationism ran directly against the interests of the consuming masses in the Northeastern and Middle Western cities; the ethnic minorities distrusted Bryan's pietism and agrarianism; and in consequence the Democratic party, which formerly had been strong in the Northeast and Middle West, shriveled in those regions. Urban ethnic groups surged toward the Republicans, who during the depression preached even more enthusiastically their traditional message that the government should actively intervene in and stimulate the economy. At the same time, William McKinley damped down the prohibitionist and nativist elements in his party so as no longer to offend the German Protestants.

Among the ethnic minorities only the Irish Catholics remained unshakably loyal to the Democrats, Bryan and all, which made that party in the North more Irish Catholic than ever. German Lutherans shifted their balance to the Republican party, where they joined the British, Scandinavians, new Dutch, Canadian British, and Yankees who had already swelled the Republican ranks to create a broad coalition of white, Anglo-Saxon Protestant peoples. The years of the fourth or Progressive Era party system, which endured from 1894 to 1930, were to be pre-eminently the years of Northern WASP America, in politics, law, and government, in the arts and the world of learning, in the national economy, and in the values which comprised the predominant national creed. Certainly Anglo-Saxonism as an almost tribal mood flourished in Republican foreign policy. Meanwhile, the Northeast and Middle West became almost as solidly Republican as the South was Democratic.³²

Terrill, *The Tariff, Politics, and American Foreign Policy, 1874-1901* (Westport, Conn., 1973); Robert D. Marcus, *Grand Old Party: Political Structure in the Gilded Age, 1880-1896* (New York, 1971); Berthoff, *British Immigrants in Industrial America*; Thomas N. Brown, *Irish-American Nationalism, 1870-1890* (Philadelphia, 1966); and Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850-1900* (2d ed.; New York, 1970). Kleppner also has a book in preparation on the third party system.

³² Samuel T. McSeveney, *The Politics of Depression: Political Behavior in the Northeast, 1893-1896* (New York, 1972); Kleppner, *The Cross of Culture*; Jensen, *The Winning of the Midwest*; McFarland, *Mugwumps, Morals and Politics*; Blodgett, *The Gentle Reformers*; Carl N. Degler, "American Political Parties and the Rise of the City: An Interpretation," *JAH*, 51 (1964): 41-59; Frederick C. Luebke, *Immigrants and Politics: The Germans of Nebraska, 1880-1900* (Lincoln, Neb., 1969), 181-83; E. Digby Baltzell, *The Protestant Establishment: Aristocracy and Caste in America* (New York, 1964); David Healy, *U. S. Expansionism: The Imperialist Urge in the 1890s* (Madison, 1970); James L. Holt, *Congressional Insurgents and the Party System, 1909-1916* (Cambridge, Mass., 1967); Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (1965): 7-28, *Critical Elections and the Mainsprings of American Politics*, 51-90, and "Party Systems and the Political Process," in Chambers and Burnham, *The American Party Systems*, 298-304; and Joel Arthur Tarr, *A Study in Boss Politics: William Lorimer of Chicago* (Urbana, 1971).

The years 1900 to 1916 witnessed that sweeping reordering of national life which we call the Progressive Era, and about which historians have been at odds for a number of years. Clarity arrives, I believe, when we examine progressivism against the paradigm provided by the republicanism of the Revolutionary period. Progressivism, like republicanism, was a broad political consciousness—distrustful of links between wealth and government and obsessed by corruption—which characterized an entire generation in rebellion against an older regime and culture. It existed as well in the four modes we observed in Revolutionary republicanism. As a holy crusade to purify American government and revive democracy, its enemy being the political party itself, it was Yankee Republican in spirit. Traditional and moralistic, Yankee progressivism ranged itself against ethnic political machines and ways of living on the one side and against the plutocracy on the other. As a search for order, to be achieved by means of strong administration under elite, expert authority, progressivism was nationalist Republican—that is, Hamiltonian. Intellectually cosmopolitan, not particularly religious, nationalist progressivism was efficiency-oriented. Led by the new university-trained WASP middle class, it was in essence a modernizing crusade seeking rationality and productivity. Again reminiscent of Alexander Hamilton, nationalist progressivism was linked to the building of military power and coherence by strengthening the navy and creating a general staff for the army. Progressives aspired to make America a powerful and active nation in world affairs. In Theodore Roosevelt's New Nationalism, this mode of progressivism found its clearest expression.

Insofar as progressivism was a movement to break apart the increasingly constrictive system of trusts and big corporations and to revive an older, individualistic way of life, it was Southern Democratic and localistic. This mode of progressivism echoed the traditional Jacksonian and Jeffersonian cries against financial power and economic privilege and against English money and the Bank of England. The classic statement of this economic libertarianism was Woodrow Wilson's New Freedom. In its egalitarian mode progressivism was urban ethnic; that is, it was Irish and German Catholic Democratic and it leaned strongly toward collectivism by drawing upon European models of social welfare. As a search for a more equal sharing in the promise of American life, egalitarian progressivism was expressed in urban social programs relating to housing, education, and old-age pensions. Northern intellectuals and urban ethnic political machines began enacting such programs at the state and local level from 1910 onward. As a result the link between the working classes and the Democratic party became ever more pronounced.³³

³³ For a rather long time historians have been in some confusion about how best to view the Progressive Era. A number of competing perceptions of that period present themselves. If the paradigm of republicanism is sound and if it did—as I think—exist in several different modes, then the varying interpretations of progressivism which we now have at hand are simply explorations of its different modes. In this regard I have long thought it unfortunate that our concentration upon organizational questions has obscured progressivism's large component of moralism and religiosity. Among the recent general accounts, only Otis

Woodrow Wilson made America's entrance into the First World War the climax of progressivism. The energies and skills built up in the revitalization of democracy at home under his leadership turned to the cause of world regeneration. Wilson was the last president to have close Scottish and British Liberal ties, and he drew his internationalist foreign policy directly from his idol, William E. Gladstone. One remembers that the Liberals, like the Democrats in the United States, were the party of the outgroups—of the Scots, Welsh, and Irish, of the non-Anglican religious Dissenters and the labor unions. Once again there is a striking congruence between cultural constituency and national policy. Gladstonian and Wilsonian internationalism emerged from political parties which had long sought equal status for minority groups. It is hardly coincidental that Liberal-Democratic internationalism argued for the equality of all nations, large or small, for a multilateral world democratic order to replace dominance by a few great powers, for the right of each people to govern themselves, and for an interdependent world economy freed of protective tariffs and therefore of special privilege.³⁴

L. Graham, Jr., would appear to have picked up this older theme again, if briefly, in his *The Great Campaigns: Reform and War in America, 1900–1928* (Englewood Cliffs, N.J., 1971), 15: The Progressives' "literature is saturated with . . . an intensely moralistic temper. This is because the generation was a religious—primarily a Protestant—generation, and the only language they knew to express their aroused feelings was the hortatory language of the Gospel—Uplift, Redemption, Conversion, Battles for the Lord, Forces of Evil, Crusades, and so on. Not only was their language drawn straight from the pulpit . . . but most of their campaigns resembled revivals. In 1912 the Progressive Party of Theodore Roosevelt, with some of the most advanced and sophisticated minds of the era in the auditorium, stood and sang without reserve 'We Stand at Armageddon and We Battle for the Lord.'" Certainly his picture, too, of the agonized Progressive sitting in his study and wrestling endlessly with ideas and moralisms reminds us of John Adams and his generation. Also see his *An Encore for Reform: The Old Progressives and the New Deal* (New York, 1967). Like Graham though with a narrower focus, Ferenc M. Szasz has brought us back to this older conception in his "The Progressive Clergy and the Kingdom of God," *Mid-America*, 55 (1973): 3–20. James L. Holt's *Congressional Insurgents and the Party System, 1909–1916* skillfully traces in the Yankee impulse as it emanated from its Middle Western precincts, and much the same spirit appears in David P. Thelen's *The New Citizenship: Origins of Progressivism in Wisconsin, 1885–1900* (Columbia, Mo., 1972). Of course, in his "status revolution" thesis, Richard Hofstadter (*Age of Reform*) focused upon the Yankees in politics, and especially upon their conflicts with corporate America and the new ethnicity. The growing literature on the suffrage and prohibition movements explores other aspects of the Yankee temper and mood. For progressivism's nationalistic and organic mode, see Robert H. Wiebe, *The Search for Order, 1877–1920* (New York, 1967), and such works as Samuel Haber, *Efficiency and Uplift: Scientific Management in the Progressive Era* (Chicago, 1964); Samuel P. Hays, *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement, 1890–1920* (Cambridge, Mass., 1959); and James Weinstein, *The Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968). The literature on Theodore Roosevelt abundantly makes clear his Hamiltonian involvement with nationalist progressivism in domestic and foreign affairs. Paolo E. Coletta's several volumes on William Jennings Bryan—especially volume 2, *William Jennings Bryan: Progressive Politician and Moral Statesman, 1909–1915* (Lincoln, Nebraska, 1969)—and Willard H. Smith's "William Jennings Bryan and the Social Gospel," *JAH*, 53 (1966): 41–60, as well as Merrill Peterson's comments in his *The Jefferson Image*, 259–66, reveal how powerfully Bryan revived Jeffersonian ideology. C. Vann Woodward's *Origins of the New South, 1877–1913* (Baton Rouge, 1951) has long made it clear that the South was the most Bryanized section in the nation, and later works, such as Sheldon Hackney's *Populism to Progressivism in Alabama* (Princeton, 1969), are fleshing out the picture. Arthur S. Link's many volumes on Woodrow Wilson provide a rich understanding of the Liberal-Democratic mind in the South and in the Progressive period generally. For egalitarian, ethnic, and collectivist progressivism, see J. Joseph Huthmacher's *Senator Robert F. Wagner and the Rise of Urban Liberalism* (New York, 1968) and John D. Buenker's *Urban Liberalism and Progressive Reform* (New York, 1973). For other aspects of this strain of progressivism, see Irwin Yellowitz, *Labor and the Progressive Movement in New York State, 1897–1916* (Ithaca, N.Y., 1965); Melvyn Dubofsky, *When Workers Organize: New York City in the Progressive Era* (Amherst, Mass., 1968); and Philip Gleason, *The Conservative Reformers: German-American Catholics and the Social Order* (Notre Dame, Ind., 1968).

³⁴ In addition to Link's studies of Wilson's foreign policy, see N. Gordon Levin, Jr., *Woodrow Wilson and World Politics: America's Response to War and Revolution* (New York, 1968), and Laurence W. Martin, *Peace*

Ironically, however, Woodrow Wilson lost out in American politics because his war against Germany and the Treaty of Versailles turned Germans, Italians, Jews, and Catholic Irishmen against him. During the 1920s the Republican Northeast and Middle West regained firm control of Washington. An openly probusiness administration directed the executive branch, and a revived cultural assault got underway against everything alien and non-WASP. Prohibition, immigration restrictions, anti-Catholicism, the Ku Klux Klan (it flourished more in northern Republican precincts than it did in the South) rallied the last full-throated WASP campaign for cultural homogeneity in American history. As before, this campaign energized the white ethnic community and drove it en masse into the Democratic party. Those hundreds of thousands of new-stock ethnics who had been arriving since the 1890s—Jews, Italians, Poles, Czechs, Slovaks, Russians—first began voting in huge numbers in the 1920s, and they instinctively lined up behind the Democrat Al Smith, the archetypical New York Irish Catholic. The Germans were also infuriated by prohibition, which they regarded as an insult to themselves, derived from the anti-German sentiments of wartime, and they too flooded toward the Democrats.

Then came the second great economic earthquake in American history. Swept by this holocaust, almost every voting group turned Democratic. The WASP coalition broke apart, as even Scandinavians, who were legendary Republicans, joined Catholics and Jews in voting for F.D.R. in cities like Chicago. In 1932, Franklin Roosevelt received almost as large a vote in the countryside and smaller towns as in the metropolitan areas; and Herbert Hoover salvaged only four states, two in that most ancient of anti-Democratic strongholds, New England, and two more in the Middle Atlantic region. By 1936 Roosevelt's voting base was so enormous that only Maine and Vermont were left in the Republican column. The fourth or Progressive Era party system was dead. The fifth, or New Deal party system, which endures perhaps even today, had arrived.³⁵

THE NATION'S POLITICS HAD COME FULL CIRCLE. Essentially, the fifth party system re-established the first. Once again in the White House there was

Without Victory: Woodrow Wilson and the British Liberals (New Haven, 1958). For Gladstonian foreign policy, its Cleveland era following in the United States, and its post-1900 expression in British politics, see Robert Kelley, "Asquith at Paisley: The Content of British Liberalism at the End of Its Era," *The Journal of British Studies*, 4 (1964): 133-59, and *Transatlantic Persuasion*.

³⁵ Allan J. Lichtman, "Critical Election Theory and the Reality of American Presidential Politics, 1916-40," *AHR*, 81 (1976): 317-51, disputes the view that the election of 1928 was "critical." On the period in general, see David Burner, *The Politics of Provincialism: The Democratic Party in Transition, 1918-1932* (New York, 1968); Louis L. Gerson, *The Hyphenate in Recent American Politics and Diplomacy* (Lawrence, Kan., 1964); Joseph P. O'Grady, ed., *The Immigrant's Influence on Wilson's Peace Policies* (Lexington, Ky., 1967); John M. Allswang, *A House for All Peoples: Ethnic Politics in Chicago, 1890-1936* (Lexington, Ky., 1971); Don S. Kirschner, *City and Country: Rural Responses to Urbanization in the 1920s* (Westport, Conn., 1970); Frederick C. Luebke, *Bonds of Loyalty: German-Americans and World War I* (Dekalb, Ill., 1974); Carl N. Degler, "A Century of the Klans: A Review Article," *The Journal of Southern History*, 31 (1965): 435-43; John Higham, *Send These To Me: Jews and Other Immigrants in Urban America* (New York, 1975); and John L. Shover, "The Emergence of a Two-Party System in Republican Philadelphia, 1924-1936," *JAH*, 60 (1974): 985-1002.

a luminous Jeffersonian national leader who talked of the common folk being exploited by special interests, distrusted bankers, praised farming and country life as vital to the nation's spirit, who was skeptical of orthodox opinion, and who was warmly supported by the ethnic minorities and the cities. Once again it was a coalition of white Southerners linked to Northern ethnics and farmers that governed the country. Like Jeffersonian republicanism, New Dealism was a broad political consciousness, so widely shared that it approached being a national consensus, ranged against a plutocratic Toryism Democrats said was to be found among the wealthy and the powerful (i.e., the predatory business interests) that supported the Republican party.

The New Deal was openly libertarian. Cultural behavior was left to personal choice. F.D.R. did not argue with urban ethnic life-styles, and Southern whites were allowed to treat Southern blacks as they wished—that is, to keep Jim Crowism and lynching fully alive as the twin vehicles of white supremacy. New Dealers were a young shirt-sleeved crowd, self-consciously brainy and professorial, whose irreverent style offended WASP America. The New Deal was also egalitarian. A folkish democratic renaissance erupted in the arts as well as in politics, and excluded ethnic groups came out of the political shadows. A government which had been overwhelmingly WASP became multi-ethnic and pluralistic.

The crucial fact about the New Deal, however, was its movement beyond these traditional Democratic modes of ideology to pre-empt the active government, or nationalist, position as well. In this the Roosevelt Democrats recreated the Jeffersonian Republican pattern, except that now the nationalist strategy was turned to frankly egalitarian purposes. The New Deal gave organized labor crucial support in its struggle with capital, established for most workers a floor under wages and a ceiling over hours, and created a broad social welfare state, in direct line of descent from the urban liberalism which the ethnic machines had inaugurated before the First World War. The Roosevelt administration even experimented with national planning—a modern version of the organic nationalism of the Puritans—though the experiment soon degenerated into specialized planning programs in aid of specific interest groups. The result was to leave many of the truly unfortunate and helpless, such as sharecroppers and migrant workers, in the discard.³⁶

³⁶ The literature on Roosevelt is legion. On voting groups and general voting patterns, see especially Arthur M. Schlesinger, jr., *The Politics of Upheaval* (Boston, 1960), 385–443; V. O. Key, Jr., *The Responsible Electorate: Rationality in Presidential Voting, 1936–1960* (Cambridge, Mass., 1966); Frank Freidel, *Franklin Roosevelt: Launching the New Deal* (Boston, 1973); William E. Leuchtenburg, *Franklin D. Roosevelt and the New Deal, 1932–1940* (New York, 1963); Lawrence H. Fuchs, *John F. Kennedy and American Catholicism* (New York, 1967), 122–25; James T. Patterson, *Congressional Conservatism and the New Deal: The Growth of the Conservative Coalition in Congress, 1933–39* (Lexington, Ky., 1967); Otis L. Graham, Jr., *Encore for Reform, and Toward a Planned Society: From Roosevelt to Nixon* (New York, 1976), 1–68; Hofstadter, *Age of Reform*, 272–328; Charles C. Alexander, *Nationalism in American Thought, 1930–1945* (Chicago, 1969); Irving Bernstein, *Turbulent Years: A History of the American Worker, 1933–1941* (New York, 1970); Raymond Wolters, *Negroes and the Great Depression: The Problem of Economic Recovery* (New York, 1970); Lubell, *The Future of American Politics*; Monroe Lee Billington, "Roosevelt, the New Deal, and the Clergy," *Mid-America*, 54 (1972): 20–33, and *The Political South in the Twentieth Century* (New York, 1975), 1–80; Christopher G. Wye, "The New Deal and the Negro Community: Toward a Broader Conceptualization," *JAH*, 59 (1972): 621–39; and David J. O'Brien, *American Catholics and Social Reform: The New Deal Years* (New York, 1968). Alonzo L. Hamby, ed., *The New Deal: Analysis and Interpretation* (New York, 1969), provides an excellent recent overview.

The Republicans had to be satisfied with the leftovers. Never again could they claim to be the spokesmen of active government, of the concept of strong national authority. They took up instead the old Democratic slogans of *laissez faire* and localism, mourned the passing of economic libertarianism, and became the voice of economic as well as cultural tradition. Now it was the Republicans who looked backward in dismay, in their case to the golden years of WASP pre-eminence. It was the Democrats who were future-oriented, inspired by a belief that they were opening a bright new tomorrow for the nation.

Here we probably touch upon the reason why the Republicans, unlike the Federalists, did not expire. The Federalists died not only because the Jeffersonians had pre-empted their nationalism, but because their pro-English feelings made them appear disloyal. Now, however, it was the Democrats who could be linked with a hated foreign power and ideology—that of Communist Russia. They were constantly accused of listening to Moscow, of being disloyal to true Americanism in their New Deal and in their internationalism. Especially after 1948, when Harry Truman frustrated Republican hopes for the White House, an assault against Democratic patriotism was unleashed which went on year after year and served to revive the Republican party. In 1800, when the enemy was aristocracy at home and England abroad, the nation had turned to the Jeffersonians and rejected the Federalists. Now the enemy was collectivism at home and Russia abroad. The country turned, therefore, to Federalism's heirs, the modern Republicans, and rejected the Democrats. Behind their war hero—Dwight Eisenhower, their own Andrew Jackson—the Republicans began to revive a two-party system in the South by awakening the old Whig coalition in that region. They took control of both the White House and Congress in 1952 for the first time since 1928. A functioning national two-party system was again in full operation, much as in the Jacksonian years.

Everything was running in Republican directions. The question that obsessed the nation was no longer economic, it was deeply and fundamentally cultural. Was it being destroyed from within by Communism? Had that ungodly force actually taken over the schools and the churches, the State Department and the army? When cultural fears predominate, Republicans traditionally benefit; as the party of the host WASP culture they seem to be the possessors of all that is essentially American. Furthermore, there were no ethnic groups to be offended by this assault upon Democratic patriotism, for in this case the foe was godless Russia, the enemy of Poles, Germans, Hungarians, Czechs, Slovaks, and all Catholics.

As the fears of an internal Communist conspiracy slowly subsided, however, the fundamentally shrunken base of Republicanism revealed itself, for the Democrats reassumed control of Congress in 1954 and have yet to relinquish it. By choosing an Irish Catholic, who could not only re-energize the classic ethnic coalition, but pull in as well the newest low-status ethnic group to begin voting in significant numbers (i.e., Catholics of Latin American extrac-

tion), the Democrats in 1960 won one of those narrow presidential victories which were so characteristic of the Jacksonian party system.³⁷

Now began the volcanic 1960s. The decade opened with a startling burst of national enthusiasm, stimulated by the charismatic John Kennedy and the hopes he aroused. The Cuban missile crisis of 1962, which in its outcome was taken by millions at home and in the world at large as ending the threat of atomic war and, in effect, the cold war, dissolved the brooding sense of national danger which had held the American people together in relatively disciplined ranks for more than twenty years. The result was an explosion of cultural change that sent waves of alarm through much of the American population, while it concomitantly encouraged those who were responding to the worldwide revolution of rising expectations to make ever bolder assaults upon the system. The surge of black America toward equality, first in the South and then in the North and West, transformed the whole nature of American life, calling up in its train the youth rebellion and, eventually, women's liberation. Under John Kennedy and Lyndon Johnson, the laws enacted in behalf of black Americans in this Second Reconstruction were so sweeping that the Democratic party became as identified with "blackness" in the national mind as in the nineteenth century it had been identified with Catholicism.

In short, the Democrats had become in the matter of race relations the morally activist party that, formerly, the Republicans had been. Whites could no longer use public authority to discriminate against blacks, and as a result the totality of American politics became, in a certain sense, Southernized. Rocked by massive disorders in the cities and frightened at the swiftly rising crime rate, which in traditional fashion had been linked to minorities, the white majority in the Northern and Western states began to respond to the same white supremacy beat that had for generations been the basic rhythm of Southern politics.³⁸

The cultural issues that have always aided the Republican party in American political history reappeared cast in new forms: an alarm over the use of drugs which formerly was directed at alcohol, distaste over new sexual mores

³⁷ Earl Latham, *The Communist Controversy in Washington: From the New Deal to McCarthy* (Cambridge, Mass., 1966); Alonzo L. Hamby, *Beyond the New Deal: Harry S. Truman and American Liberalism* (New York, 1973); George Brown Tindall, *The Disruption of the Solid South* (New York, 1972); Billington, *The Political South in the Twentieth Century*, 131–85; Herbert S. Parmet, *The Democrats: The Years After FDR* (New York, 1976); O'Brien, *American Catholics and Social Reform*, 70–96; Charles C. Alexander, *Holding the Line: The Eisenhower Era, 1952–1961* (Bloomington, Ind., 1975); James T. Patterson, *Mr. Republican: A Biography of Robert A. Taft* (Boston, 1972); David M. Oshinsky, *Senator Joseph McCarthy and the American Labor Movement* (Columbia, Mo., 1975); Michael P. Rogin, *The Intellectuals and McCarthy: The Radical Specter* (Cambridge, Mass., 1967); Gary W. Reichard, *The Reaffirmation of Republicanism: Eisenhower and the Eighty-Third Congress* (Knoxville, Tenn., 1975); Fuchs, *John F. Kennedy and American Catholicism*; and Mark R. Levy and Michael S. Kramer, *The Ethnic Factor: How America's Minorities Decide Elections* (New York, 1973).

³⁸ I shall make no effort systematically to provide references to relevant literature concerning the 1960s, save briefly: William L. O'Neill, *Coming Apart: An Informal History of America in the 1960s* (New York, 1971), and Ronald Berman, *America in the Sixties: An Intellectual History* (New York, 1968). Numan V. Bartley and Hugh D. Graham, *Southern Politics and the Second Reconstruction* (Baltimore, 1975), is an essential new study which examines in great depth local politics in each region of the South and relies upon massive, freshly gathered quantitative documentation.

and styles of dress, fears engendered by violence in the streets, and disruptions of the national culture brought about by the emergence of powerful, newly self-conscious ethnic groups hitherto relatively without influence. Though the economic and institutional power structure remained WASP, the nation's cultural life began to shift rapidly in non-WASP directions, in literature, the arts, and scholarship; in music, the movies, and television; in dance, dress, hairstyle, and cosmetics; and in ordinary speech as obscenities once tabu became tolerated in everyday discourse. New idioms, new accents, new values, the death of the *Saturday Evening Post* and *The American Magazine*—it was a new and alarming world.³⁹

The first Republican response offered us Barry Goldwater in 1964, but his social conservatism at home and militant aggressiveness abroad were too uncut and too raw; and the full cultural upheaval of the 1960s was yet to appear. Then came Lyndon Johnson, pouring out his cornucopia of social reforms and mounting his increasingly violent war. The nation got its fill of missionary diplomacy and foreign adventurism, lost confidence in its own moral purity in foreign affairs, and finally learned that there are limitations even to America's capacity to work its will in the world. In a general revulsion against the seismic cultural upheavals of the 1960s, the national electorate swung massively to the right. The two-party South re-emerged to vote heavily Republican; even Catholic voters of European (not Latin American) descent, in a stunning reversal of a pattern as old as the nation itself, tipped in the same direction. With almost two-thirds of the immense white Protestant group voting for right-wing candidates in 1968 and 1972, we had delivered to us, finally and at long last, Richard Milhous Nixon.⁴⁰

CULTURAL EXPLANATIONS WORK WELL when dealing with masses of people. With individuals, the situation is more complicated, for personal motivations are notoriously elusive. We shall never know exactly why President Richard Nixon did such appalling things in Vietnam and Cambodia or in Washington during his tortured maneuverings against his political enemies. But in the

³⁹ The concept of the "WASP," used extensively in this paper, seems to have been a new product of the last decade or so: "The newly minted term WASP," writes John Higham, "became in the 1960s the only ethnic slur that could safely be used in polite company; for it was part of a largely successful assault on certain remaining bastions of ethnic exclusiveness. . . . The earliest references I have found occur in 1964. In Saul Bellow's exuberant novel, *Herzog* . . . , Moses Herzog relishes 'defying the Wasps, who . . . stopped boiling their own soap circa 1880, took European tours, and began to complain of the Micks and the Spicks and the Sheenies.' In the same year and in the same sense E. Digby Baltzell introduced the term into the vocabulary of American social science in *The Protestant Establishment*. . . . Quickly, in writers like Peter Schrag and Michael Novak, 'Wasp' became synonymous with a desiccated, life-denying culture" (*Send These to Me*, 13). Also see Peter Schrag, *The Decline of the WASP* (New York, 1971), and Michael Novak, *The Rise of the Unmeltable Ethnics: Politics and Culture in the Seventies* (New York, 1972).

⁴⁰ John H. Kessel, *The Goldwater Coalition: Republican Strategies in '64* (Indianapolis, 1968); Bernard Cosman and Robert J. Huckshorn, eds., *Republican Politics: The 1964 Campaign and its Aftermath for the Party* (New York, 1968); Lawrence H. Fuchs, ed., *American Ethnic Politics* (New York, 1968); Edgar Litt, *Beyond Pluralism: Ethnic Politics in America* (Glenview, Ill., 1970); Levy and Kramer, *The Ethnic Factor: How America's Minorities Decide Elections*; Perry L. Weed, *The White Ethnic Movement and Ethnic Politics* (New York, 1973); Lewis M. Killian, *White Southerners* (New York, 1970); and Stephen D. Isaacs, *Jews and American Politics* (Garden City, N.Y., 1974).

larger outlines of his politics, as indeed in the case of most politicians, much becomes at the least more reasonable and understandable if we place him against his cultural background. That both of our Quaker presidents have been Republicans is congruent with what we know, historically, of that gentle but stern faith. In all of American politics, few peoples have been more wedded to Republicanism than the Quakers; few have more consistently disliked Democrats. Tory in the Revolution, they were Federalist and Whig thereafter. In the 1850s, they helped found the Republican party. Their Hicksite minority was abolitionist, and the Orthodox Quaker majority was pietistic and moralistic. Questions of life-style were absolutely crucial to the Quaker mentality. An austere people who frowned upon music even in the home, abstemious, hard working and soberly prosperous, respectful of authority and hostile to disorder in any form whatever, they were ethnically English, the very model of the WASP cultural archetype which lay at the core of Republicanism. To them, Democrats were epitomized in the town drunk, in the irreligious radical, and in the alien Catholic. During the Gilded Age, they voted eighty-five percent Republican, a proportion which made them among the truest of true believers.

Never numerous, they had been known since colonial days for their "Quaker tribalism." Team spirit was the breath of life. Wherever they congregated, their settlements were inward-turning cultural enclaves. Whittier, California was a Quaker colony, as Republican as Harlem is Democratic. Richard Nixon grew up in a traditional small American town, one which reared culturally and politically conservative young people with complete naturalness. He was early taught to regard Democrats as morally corrupt, almost un-American, and he was closely trained in a hard working, self-denying, get-ahead mode of life centuries old. In 1920 at the age of seven, he stood in Whittier's streets and argued passionately for Warren Harding. As a politician, his career was to demonstrate with almost eerie precision the precarious base to which his party had been reduced, for he had only one mode: he was the consummate moralist in politics. Cultural politics in domestic affairs was his only stock in trade.⁴¹

Richard Nixon built his first political career entirely upon the assertion that the Democrats consorted with traitors, that America was being assaulted

⁴¹ Kleppner, *The Cross of Culture*, 70, for Quaker voting percentage. "Quakers, perhaps more than evangelicals," Ronald Formisano observes of the Friends in the Jacksonian years, "did not compartmentalize their lives into secular and religious sectors. They did not share in revivalism but their . . . intense pietism and desire for a moral society led them to vote overwhelming for Whig and antislavery parties" (*Birth of Mass Political Parties*, 149). The Milhouses were a great beehive of a family, profoundly Quaker for generations, and they simply absorbed the wandering Frank Nixon, Richard Nixon's father. "They were staunch Republicans, these Milhouses of Jennings county [Indiana]," writes Edwin P. Hoyt. When they moved to Whittier, California, in the 1890s, "The place was completely dominated by the Quakers. There were no saloons or beer halls in the town, no atmosphere of harum scarum that characterized so much of the Los Angeles area. The people who settled here at the base of the mountains were God-fearing Friends, the kind of people among whom the Milhouses had moved for six generations. There was no absolute way for the Quakers to insulate themselves against a rough and thoughtless society, but the Friends controlled Whittier." *The Nixons: An American Family* (New York, 1972), 164-67. Frank Nixon was of Scotch-Irish lineage and had been a militant Republican partisan since youth.

internally by Communism. In the years after his narrow defeat in 1960 he fell into apparent oblivion and then rose again on the crest of the violently cultural politics of the late 1960s. Crying law and order and attacking the rebelliousness of youth for all the world like the crustiest of Federalists in Thomas Jefferson's time, he won the White House in 1968. The booming earthiness of Lyndon Johnson was instantly replaced by sober coats and well-bred public manners. Neatness and closely cropped hair styles, efficiency and order—these were the Nixonian hallmarks. The enemies were clear: agitators, law breakers, hippies, the culturally deviant. Intellectuals, Jews, blacks, writers and artists, certainly the disorderly and irreverent members of the press—these comprised the opposition.

It was, of course, a business administration. As he told his attorney general, there was to be no damned trust-busting around here. Nixon's actual ties to the corporate boardrooms, however, seem in fact to have been rather weak. Instead, it was team-spirit Americanism that obsessed him. Police forces and national guard organizations looked to him as their natural leader, for he preached that authority and a strong hand, rather than social reforms, were what the nation needed. The new theme in foreign policy was withdrawal from entanglements, even those with apparent friends, and standing alone. Thus was revived the nationalist, unilateral foreign policy that his political tradition preferred. When Nixon looked to Britain for inspiration, he found it not in Gladstone but in the great Tory premier, Benjamin Disraeli, who had also aspired to the lonely, imperial hand in foreign relations. When the crisis of the American dollar struck in late 1971, Nixon moved immediately in nationalist directions. He clapped on surcharges which amounted to protective tariffs and took the dollar off the international gold standard, placing it under the United States' national control. In all of this he acted peremptorily, after no significant discussion with friends and allies.

The two major political traditions in this country embody broad spectra of belief and action. A William McKinley will take his place at one point on the Republican spectrum, a Theodore Roosevelt at another. Richard Nixon took his place toward that end where the Republican tradition is still essentially Federalist, indeed Tory, in spirit. Thus, if Franklin Roosevelt reawakened the Jeffersonian atmosphere and party system, Richard Nixon revived the Revolutionary crisis which preceded it. Conceptions reaching back to the Tory governments of eighteenth-century Britain appeared to come fully alive again in the twentieth-century White House: that legislative bodies were irritating institutions whose views could be set aside and ignored; that the Crown had an unchecked prerogative to do what it felt was best for the nation, whatever the laws said; that criticism was identical to disloyalty; that rulers should be remote, inaccessible, almost divine; and that disorder and rebelliousness had to be crushed with a strong hand.

However the American revolutionaries may have later divided in their politics, when they looked at Tory Britain they were all Whigs. The revolutionaries wanted a limited executive operating within laws established by the

people's legislators. Thus, when Richard Nixon began moving across the fundamentally Whiggish grain of the constitutional system, it rose up to cast him down.⁴²

WHAT REFLECTIONS ARISE FROM CONSIDERING THESE TWO CENTURIES of American political history? First, one is struck by the great age and durability of the party system and the ideologies to which it gives expression. Two clusters of peoples and ideas coalesced against each other in the Revolutionary generation, and with slow modifications and accretions they appear to have endured to the present day. The pattern then established has survived for two hundred years. We have a long tradition in this country of believing that parties have little meaning—historians, indeed, have only recently begun to pay them serious attention—but this is not so. There is, of course, much intermixture and overlapping between the two parties. Individual voters are influenced by a plurality of motives, and many cast ballots which apparently do not align with their ethnicity and economic interest. The Republican rabbi and the rich Democrat are stock figures in politics. One must speak, therefore, of centers of gravity in a mass political party—of overall thrust, character, and mood—rather than of two mutually exclusive and clearly distinct organizations. With this caveat, however, it seems on the present record clear that the Democrats and Republicans and their lineal forebears have been different kinds of people and that their leaders have held correspondingly different visions of what the nation is and should become.

These patterns of party membership and ideology have been, in their larger outlines, reasonable and coherent. They make both logical and historical sense. Most of all, the parties have been shaped by a feeling that their members share a common enemy. This basic sense of an adversary relationship, the strongest force binding parties together and giving them meaning, can be rooted in immediate economic circumstance or in cultural rivalries and animosities sometimes centuries old. Precisely because the cultural substructure of politics is so deep, so bred into each of the nation's peoples that it seems a part of their bone and marrow, there have been few changes in ideology, even over two centuries.

Thus, if Thomas Jefferson were to be brought back to life today, he would have little difficulty seeing where he belonged. The Democratic party is still the party of the outsiders, of the ethnic minorities who are relatively low in cultural and economic status or who in other ways have been made to feel excluded by the host culture. Such outsiders gravitated to Jefferson because his ideology of equality and of personal liberty to be different—an ideology of

⁴² One is largely unable to cite scholarly works on this most recent period and the Nixon presidency. My account of it is drawn from the wide variety of public materials to which we have all been paying close attention in the last eight years. Of especial value is Arthur M. Schlesinger, jr., *The Imperial Presidency* (Boston, 1973), a work destined, one judges, to be accounted a classic of the period. On the Nixon coalition, see Kirkpatrick Sale, *Power Shift: The Rise of the Southern Rim and Its Challenge to the Eastern Establishment* (New York, 1975).

cultural as well as of economic *laissez faire*, which in both instances aimed at eliminating privilege—ministered directly to their needs, both psychological and material; and they adhere today to the party of his tradition for the same reasons. With this voting base, the Democrats continue to conceive of themselves as the party of the poor, as well as of those with middle incomes—that is, of the consuming classes in general as against the merchant, the producer, and the financier. And the outsiders' characteristic paranoia is still Jefferson's: that economic conspiracies among the rich and powerful constantly act to exploit society at large and that business and government ties lead to political corruption, the ancient evil that obsessed the Jeffersonian mind. From Democrats we continue to hear Jefferson's attacks upon economic privilege, corporations, and rapid, speculative development. Environmentalism, an outlook strongest within their party, is one modern form of this brooding distrust of industrialism and modernization.

From Democrats, too, we hear the most about cutting the defense budget, in Jeffersonian fashion, and about the dangers of militarism. Even in these times of reluctance to take clear stands in foreign policy, one senses that Wilsonian internationalism with its paradoxical blend of multilateralism and missionary diplomacy—the urge to improve the world—still resides, ascendant, in the Democratic consciousness (as the revived campaign for human rights in the world by the Carter administration now demonstrates). The Nixon-Kissinger performance hints that a balance-of-power unilateralism, nationalist and military in the spirit of Theodore Roosevelt as well as, to a lesser extent, business-centered in the spirit of William Howard Taft, continues to claim precedence in the Republican outlook. The Vietnam tragedy belonged to both parties, but their feelings toward it—agonized and ambivalent on the one side, preponderantly stern and determined to the end on the other—once more revealed their different natures.

The cultural image of the Democratic party remains strikingly distinctive. Thomas Jefferson was condemned for his unbuttoned and rather free-swinging personal style as well as for not being much concerned about popular disorder, and an aura of permissiveness still hangs about his political inheritors. Certainly the issue of law and order consistently has Democrats on the defensive. Certainly, too, they are associated with rebellious youth, as in Jefferson's time, in a way never attributed to Republicans. Free-thinking intellectuals—irreverent and skeptical, playful with ideas held dear to multitudes—continue to form in the Democratic party a functioning if not wholly comfortable partnership with Baptists and Roman Catholics. From this association of intellectuality with Democratic partisanship springs that reputation for radical sympathies that Jefferson would recognize.

The Republican party, like the Federalist and the Whig parties before it, continues to be the political expression of those who look upon industrialism and government-sponsored economic development with approval, who regard the entrepreneur and the employer not as potential buccaneers who must be restrained but as the creators of social improvement and well being. Republi-

cans instinctively see the labor movement as presumptuous and obstructive. Friendly toward producers, the Republican tradition in the spirit of Henry Clay is keenly concerned with insuring good climates for investment and impatient with those who in a spirit of watchful distrust wish to regulate and hobble the business system. Defining the boundaries of free enterprise much more widely than Democrats, Republicans still believe that, left to itself, the business system will do far more to solve the nation's social problems than will the government.

Gerald Ford is an archetypical Northern WASP from a classically Republican Dutch constituency in western Michigan. His party remains the vehicle of those who think of themselves as, and are generally thought to be, the host culture—that is, of those who originally comprised English or Yankee America and in time became WASP America. Thus, within Republican ranks are gathered most of those who believe that they are the special guardians of true patriotism, true Americanism. Their characteristic paranoia is to believe that political conspiracies are constantly at work to weaken and subvert the nation. Primarily Republican, too, are those most ready to believe that moral corruption in the life-styles of those not like themselves threatens to destroy virtue and must be fought by making the government a moral policeman. It was the Republican national convention in the last election that busied itself with abortion and drugs; it was the Democratic candidate who refused to support a constitutional intervention on abortion and even talked about sex in a girly magazine. As it has been from the beginning, the Republican image is that of orderliness, respect for authority, sober good taste, self-control, and avoidance of the flamboyant and the earthy. The diminishing of the Republicans as a party in the Congress since 1954 may reflect a similar dwindling of the WASPs themselves in numbers and influence or, conversely, a diffusion of this Northern WASP life-style and identity in the population, a diffusion so wide as to cost the WASPs a consciousness of themselves as participants in a great national rivalry. When enemies fade, so do parties.

In contrast to Gerald Ford and in the tradition of the Democratic party, Jimmy Carter is part of an ethnic outgroup excluded for more than a century from the White House. Indeed, as a Deep Southerner and evangelical Baptist, he was himself the principal cultural issue in a campaign in which, ultimately, economic questions appear to have overshadowed cultural ones. Even so, his Southern accent, Southern Baptist style, and Southern populist aura almost defeated him, so widely do these diverge from the Northern WASP manner and outlook that seem to have become the basis for a national culture.

Carter's victory seems to demonstrate that the fundamental dynamics which have characterized our politics from their beginnings are still present, for his election depended upon that same alliance between the South and the ethnic minorities of the Middle Atlantic States that placed Jeffersonians and Jacksonians in the White House. Furthermore, just as the massive pro-Jefferson vote of the Scotch-Irish, despised by the English majority of that time, put the Virginian into the presidency, so the massive pro-Carter vote of

black America, feared by the white majority in our time, delivered the presidency to the Georgian. There is a profound irony here, for until recent years the Democrats have been the white racist party; but this paradox need not disturb us unduly. An influx of newly enfranchised groups of low cultural status has traditionally aided the Democrats at crucial times in their history. In this case the Voting Rights Act of 1965, which effectively enfranchised the Southern black population, delivered what may be the last large new American voting group to the Democrats.

If the republic survives and there is to be a Tricentennial, one wonders what historians will have to recount on that occasion. We hear often that there is soon to be a major political realignment. If the Republicans are dying, as the Federalists before them passed away, can a new Whig party be far behind? By contrast, we also hear that the party system itself is dying. As evidence for this assertion, observers point to the long decline in voter participation and interest and the growth in the number of those who say they are not members of either party. The nation seems, historically, at a significant point in its long adventure with mass politics.⁴³

If we are persuaded by the record of historical continuity and persistence which has here been presented and if we believe, as I do, that cultural tribalism and economic rivalry are lasting elements in the human condition, then it is difficult to believe that party politics and ideologies, in much their present forms, will not occupy our future national life as they have our past. At present, the ideological partisan imbalance born in the New Deal years seems enduring. Having then lost their long claim to governmental activism, the Republicans continue to offer little more on the national scene than moral vigilance and hostility to social and economic intervention. Such positions are apparently not enough to sustain anything more than a reduced minority in the Congress. The cultural imbalance born in the New Deal years also persists. Banded together behind the Democratic party, the outsiders are still numerous enough to control Washington. The Democratic majority in Congress seems touched with eternity. Only in the executive branch has there been anything like a balance between the two parties for more than forty years.

It was widely said in the 1920s, however, that the Democrats were dying as a party as were the Liberals in Britain with whom they shared so much. In fact, of course, cultural shifts then not closely noticed were laying the foundation for a Democratic revival which the great Depression transformed into a total rout of the Republicans. In similar fashion, are there deep shifts now in motion which point to a different political future? If some of the outsiders who now vote Democratic become insiders, what then? Catholics of European

⁴³ For this discussion, see Burnham, *Critical Elections and the Mainsprings of American Politics*, 91–174, and his “American Politics in the 1970s: Beyond Party?” in Chambers and Burnham, *The American Party Systems*, 308–57; Richard L. Rubin, *Party Dynamics: The Democratic Coalition and the Politics of Change* (New York, 1976); James L. Sundquist, *Dynamics of the Party System* (Washington, 1973); Numan V. Bartley, “Voters and Party Systems: A Review of the Recent Literature,” *The History Teacher*, 8 (1975): 452–69; and Samuel Lubell, *The Hidden Crisis in American Politics* (New York, 1971).

descent have become the group with the highest income, per capita, in the country, and their life-style seems no longer to attract much prejudicial comment.⁴⁴ Protestants are losing their formerly virulent anti-Catholicism. With rising wealth, the Catholics seem destined to play an increasing role in the institutional power structures of the country that are now overwhelmingly WASP. Will they eventually provide that fresh increment of social strength to the Republican party, or to whatever party succeeds it, that is needed to restore a full, relatively equal two-party rivalry?

In presidential balloting, Richard Nixon pulled the Roman Catholics of European descent to his side. The latest evidence is that their migration to the suburbs does not as yet make them into Republicans, that as a group they remain more politically liberal—that is, economically and socially activist—in their attitudes than Protestants. Who can say how long this will last, or even that it will continue to hurt the Republicans? There are indications that wherever Republicans at the local level have nominated liberal candidates, they have cut sharply into the Democratic Catholic vote.⁴⁵ The political party (and I use that word in more than one sense) is not over. It continues to go on, around the clock.

⁴⁴ Andrew M. Greeley, "The Ethnic Miracle," *The Public Interest*, 45 (1976): 20–36. According to recent data, "The Italians are now the third richest religio-ethnic group in American society—second only to Jews and Irish Catholics—and the Poles earn almost \$1,000 a year more than the average white American in metropolitan areas of the North" (page 24). In *That Most Distressful Nation: The Taming of the American Irish* (Chicago, 1972), Greeley observes, "If the fundamental theme of this book is correct, . . . the . . . Irish will disappear . . . into upper-middle class suburbs." (page 255). Also see his recently published *The American Catholic: A Social Portrait* (New York, 1977) for a full analysis of the new data.

⁴⁵ Rubin, in *Party Dynamics: The Democratic Coalition and the Politics of Change*, reports that "suburbanization, as such, has not had a significant effect on the political ties of key urban Democratic groups that had migrated to the suburbs after World War II . . . voting loyalty among transplanted urban Democrats was as high if not higher than that of Democrats remaining in the major cities." On the other hand, in Eastern precincts where the Republican party has nominated more liberal candidates, he observes that Democratic loyalties among Catholics have weakened (pages 172–73).



Comments:

ROBERT KELLEY IS A BRAVE MAN. Since the Washington meeting of the AHA was designed to provide American political historians with a last hurrah for the bicentennial year, someone had to try it; and in many ways I think that he has brought it off. To anyone who has *not* tried it—myself included—the dimensions of his effort seem almost immodest. He is concerned with large themes of ideology and culture in our politics, and he plunges right in. In contrast to so many efforts at broad-gauged synthesis, his essay has not followed the safe, high-flying route above the reach of immediate verification. It respects chronology, no small virtue. It promises new vitality for the art of history as narrative. It is functionally connected to the latest journal and monographic literature. And it moves at a level of specificity—about groups, events, and individuals—which allows his critics to test the whole argument against the parts they know best. He has given us a lot to ponder, and we need to pause before we say, “Yes.” And yet I think that many of his colleagues (and readers of the book for which this essay provides a preview) will find his message quite persuasive, even alluring. A big audience may be out there, ready to say, “Yes.” To explain this hunch, I will be autobiographical.

For historians of my age—those who entered graduate school in the mid-1950s just as the certitudes of progressive history were crumbling, who learned proper attitudes of humility and irony while trying to absorb the complexities of post-progressive historiography, and who then passed through the 1960s wondering if we were either technologically obsolete for lack of computer training or ideologically musclebound for doubting the radical truths of the New Left—for mainstream general practitioners of that ilk, Kelley’s message is oddly comforting. The dust settles and the clouds roll back, and lo and behold, by the new bright light of ’76 the remembered architecture of boyhood is still there. If anything it is more imposing, more three-dimensional, and somehow more believable than ever before.

Foreign and domestic wars and great depressions still punctuate the big turning points. Important presidents still preside importantly—especially those blessed with a Scotch-Irish connection. And the old partisan divisions dear to progressive historians remain intact, colored with fresh meaning and making fresh sense. Pro-English Federal elitists lose to triumphant Jeffersonian nationalists. The Age of Jackson generates real and ideologically consistent quarrels across a long and well-marked battle line dividing Whigs and Democrats, quarrels in which the capitalist rich and working poor had vital stakes. Somewhat surprisingly Old Hickory re-emerges after the Civil War fleshed out in the bulk of Grover Cleveland—though that comes as no shock to students of Kelley’s earlier work. Not only did the Progressive movement, it

turns out, take place, but progressivism revitalized the processes of national democracy in multiple ways analogous to those by which eighteenth-century revolutionary republicanism had initially charged the nation. And the New Deal was new after all. Those textbooks of the 1940s were not putting us on.

I do not mean to parody or trivialize Kelley's grand design. What makes it so appealing, so believable, is that he has *succeeded* in doing what he set out to do: namely, to establish a close and durable mesh between what we always knew about national programmatic politics and what we have recently learned about the cultural preoccupations of the local and regionally rooted common mass. He has knit back together what appeared to many puzzled scholars across the 1960s to be a widening split between two tiers of political reality—the mundane behavioral reality of life at the grass roots and the public policy realities of the elected national elite. In the atmosphere of creative confusion spread by the behavioral history of Lee Benson and Samuel P. Hays in the early 1960s, the disparity between high-profile national rhetoric and low-profile local behavior across the long stretch between the Age of Jackson and World War I encouraged growing skepticism about the possibility of reconciling the two. Apparently the rhetoric had to go. And for a historian like myself, working in the Gilded Age, the quantitative findings of Paul Kleppner, Richard Jensen, Samuel McSeveney, and others deepened the puzzlement. What happened to tariff debate, civil service reform, and free silver if political reality turned out to be located in saloons, churches, and schoolhouses all along? How could we explain to our wives and deans those endless wasted weeks at the Manuscripts Division of the Library of Congress reading elitist rhetoric?

The remarkable consistency in the findings of the quantifiers about the ethnocultural motives of American voters and the narrow horizons of their political awareness seemed to leave students of national policy-making in a precarious plight. The quantifiers radically changed the landscape we now had to travel over, and we were not sure who had the right compass. It seemed as if the old and new schools of political history were like wagons passing in the night. But Kelley restores leaders to their constituencies and assures us that the motives of each can be integrated after all. It is the fabric of *cultural* history that does the job, uniting the perceptions of the leaders and the led. Time and again he finds ethnic and religious passions reinforcing rather than disorienting animosities aroused over issues of class and economic difference. Thus in turn each of the classic confrontations—Federalist versus Jeffersonian, Jackson men versus Whigs, slavery versus antislavery, Bryanites versus McKinleyites, WASP versus immigrant, New Deal pragmatism versus economic tradition, 1960s alienation versus backlash and even Watergate—is made to throb with insistent cultural meaning. Toward the close of his essay, Kelley suggests that Jefferson, if brought back to life, would have no trouble seeing where he fitted into the partisan configurations of late twentieth-century America. I daresay something similar might be said for Charles Beard and Vernon Parrington: Kelley's stage design, for all its subtle new

complexities, would not seriously disorient them. And they would warm to the plot.

The restoration is mostly persuasive. It accepts much of the new scholarship of the last two decades and incorporates it to verify old stories. It absorbs the structural concept of five succeeding party systems from Jefferson to F.D.R. and uses it to establish an almost cyclical continuity in the changing shape of coalitions. It employs the theme of perceived conspiracy to accentuate the reality of conflict rather than dilute it, whether the context is revolution, civil war, or cold war. It weaves Robert Merton's useful notion of cosmopolitan versus local awareness into patterns of regional difference so as to modernize Turner's vision of sectional strife. Above all it uses the concepts of ethnicity and religious identity to illuminate the behavior of old-stock natives as well as immigrants.

Kelley boldly insists that ethnocultural explanations must be applied to individual biography as well as to the analysis of large groups. Here he may be entering hazardous ground, especially as he moves out of the nineteenth century into more secular and culturally more mobile modern times, when few of our national leaders spring directly from coherent religious or ethnic community enclaves into places of public authority. Hard statistical evidence about voting shifts of large numbers of coreligionists is one thing. The penchant for discovering religious origins in the acts of individual leaders is another, and perhaps more slippery, matter. Was the Episcopalianism of F.D.R. more relevant to his enthusiasm for the TVA or to his use of the FBI against political foes? Does the Catholicism of John Kennedy better explain the Bay of Pigs or the Missile Crisis? There is, I think, a point to these silly questions. Andrew Jackson's Scotch-Irish immigrant parentage and Woodrow Wilson's Scotch-Irish Calvinism may indeed be found to have shaped their peculiar behavior at specific moments or to have informed their general mental disposition. But every biographer knows how tricky the task of integrating personality with the institutional mesh of secular power structures can be. Kelley may be right about Richard Nixon's Whittier boyhood, but I think he saddles the Quakers with a heavy burden for the sins of Watergate.

SHIFTING FOCUS SOMEWHAT ABRUPTLY, I believe that in this essay as in his earlier book, *The Transatlantic Persuasion*, Kelley helps us to understand more clearly than we did before the rich diversity of meanings in the term "laissez faire." For nineteenth-century American Democrats the term conveyed not only a set of antimercantalist imperatives about economic policies of national and international dimension—minimal intervention, minimal regulation, and minimal trade restraints. Laissez faire also communicated a domestic cultural guarantee: it promised minimal public coercion in the lives of minorities whose special mores might offend official norms. Thus it was no accident, for example, that Irish immigrants whose folkways were so abusive to native sensibilities should find a safe-house in the party of Jackson—the party that

flapped the banner of laissez faire against the mercantile nationalism of Daniel Webster and Henry Clay. Again we see with Kelley that the high-level rhetorical debate and the low-level political behavior are causally, culturally connected. Nor was it a random coincidence that in the 1880s a quiet political friendship should develop between Tammany Hall's Honest John Kelly and Delaware Senator Thomas Francis Bayard, perhaps the most rigid spokesman of laissez faire in the Democratic party of his day. The striking similarity between the laissez faire rhetoric of Grover Cleveland Democrats and the antistatist pronouncements in the papal encyclical of 1891, *Rerum novarum*, as interpreted by Irish Catholic spokesmen in Boston, revealed a broad fund of cultural as well as economic agreement between Irish and Yankee Democrats, enabling them to surmount keen ethnic mistrust and work together in the same party.

As Kelley notes, it was not really until the crisis of the 1930s that the New Deal decisively snapped the historical link between cultural and political laissez faire which had governed the national ideology of the Democratic party for most of the preceding century. The New Deal ethos, libertarian in private cultural behavior and interventionist in public economic policy, energized a party coalition which ever since has thrived on economic adversity and suffered from cultural dissonance. It may well be, as Kelley suggests in his conclusion, that the future balance of the party system will depend on the behavior of newly affluent Catholic Americans, whose tribal loyalty to the Democratic party has been a linchpin of our politics since the 1840s.

I suppose it is clear that I find Kelley's essay an impressive foray in historical synthesis. I do wonder, though, how long it will satisfy, and how many it will satisfy. For one thing, I suspect that ethnocultural politics will be found to cut against the grain of the politics of class consciousness and economic self-interest more often and with less tidy consequences than he seems to suggest. Kelley writes liberal history, in which the good guys of progressive history become cultural "outsiders" warring against the constraints of the "host culture." The vocabulary he uses to describe the politics of cultural combat and its outcome is at bottom the language of affirmation: the process "works." I doubt that historians of a radical persuasion will find this lesson very compelling—particularly in its implied assumption that our party system has been an adequate vehicle for absorbing and reflecting the full range of political dissent among Americans. The ideology of alienation among outsiders *outside the system* is an aspect of reality that does not fit easily into Kelley's definition of political culture. One might be inclined to ask of the whole American past what Walter Dean Burnham and others have been asking about the more recent past: how many Americans have turned out for Kelley's America, and what about all those people who tuned out and turned away? Kelley has reduced the gap between the cultural behavior of the electorate and the public behavior of the elected. But we must not forget that the electorate has always been a minority of the whole population, that levels of participation have varied radically among different groups, and that party

politics (thank God!) have rarely encompassed the full cultural passions of the American people.

Other reservations linger. The essay, in its search for continuities and analogies, takes rather small account of major sources of political change over time—urbanization, secularization, and the frequent victories of technology over ideology—in the resolution or compounding of public problems. It may well be that other criteria of self-identity, such as age, sex, and education, have in our postindustrial environment largely vitiated the firm ethnoreligious loyalties of a century ago. But this is not the place to badger Kelley for what he has not said in thirty pages.

While I was making up my mind about his essay last December, I came across a quotation from William James which seemed oddly pertinent. “There is no complete generalization,” James wrote, “no total point of view, no all-pervasive unity, but everywhere some residual resistance to verbalization, formulation, and discursification, some genius of reality that escapes from the pressure of the logical finger, that says ‘hands off,’ and claims its privacy, and means to be left to its own life.”¹ James’ words can be read to carry both a historical message similar to Kelley’s own and a historiographical warning for him as well. The strength of his essay derives in large part from the way it builds on the durable tension in our history between a drive for broad cultural unity and a stubborn resistance to national norms, a resistance that protects the local, the particular, the private. That part of his synthesis will last. But his essay will be vulnerable to the dynamics of historiography, which ensure that no complete generalization, no total point of view, will long go unchallenged. As Kelley notes in the final version of his essay, he knows the Jamesian risks he runs in laying hands so masterfully on the raw stuff of the past. For the time being I am happy to contemplate his reconstruction and learn from it. It is pleasant to share with him a moment of intellectual control. For some of us it has been a long wait. The time will come soon enough when the past will be heard to rumble, as it does periodically, “hands off.”

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¹ *Memories and Studies* (London, 1911), 410, quoted in Wayne Andrews, *Architecture, Ambition, and Americans* (New York, 1964), 255.



FEW HISTORIANS COULD HAVE PRESENTED THE ANALYSIS with which Robert Kelley has favored us. Those familiar with the themes of his seminal book, *The Transatlantic Persuasion*, and with the unusual character of his recent text, *The Shaping of the American Past*, will not be surprised by the bold cultural interpretation given here. Admitting that this is *one* view of United States political history, inevitably incomplete, I applaud its art and agree with its thrust. The cultural dynamics of our politics have been ignored for too long. In recent years this situation has begun to change. But, if Kelley now pushes a broadly defined ethnic factor too far, his analytic emphasis may be necessary, given past resistance to recognition of noneconomic influences on political party alignments. Of course, some of Kelley's generalizations are premature, and even he admits to guessing about some groups' behavior for lack of studies; but on balance the heuristic impact of his paper should be healthy.

The danger lies in treating Culture or Ethnicity as another Frontier thesis,¹ and rushing into belief or rejection before we have a clear idea of what we are talking about. Kelley's essay mixes together generalizations ranging widely in their empirical support, from those well-founded in several monographs to those springing from Kelley's inspired guessing. This paper also lacks precision in using concepts central to its argument: political culture, ethnicity, and ideology. The need for conceptual clarity is not mere semantics. Definition determines not only emphases but substance, especially in an essay of synthesis. Contrary to Kelley's usage, "political culture" is an inclusive term, referring to the "givens" and implicit values of a polity seen as a whole. Within any political arena, cultural politics can be understood to refer to clashes over broadly defined life-styles, symbolic groups, and beliefs, while ethnic politics has a relatively concrete reference to more durable and exclusive identity groups. Thus, cultural and ethnic politics are different phenomena. Kelley's fusion of them decreases the effectiveness of his analysis. Although his application of a cultural factor is sweeping, his understanding of how community context creates variations in social group or subcultural alignments is minimal if not absent. By calling attention to these questions, a sympathetic critic can suggest strategies for reformulating Kelley's thesis to incorporate the change and variety it leaves out. In that spirit, then, I shall try to make explicit some of his assumptions and shall conclude by considering what needs to be done.

IT MAY BE USEFUL FIRST TO CONSIDER why a cultural interpretation emphasizing ethnicity has taken so long to emerge. Historians have not been alone in

I would like to express appreciation to David H. Fischer, Rebecca Hanson, William L. Joyce, and William G. Shade for their suggestions and advice.

¹ A colleague has reminded me that on a pleasant night back in 1964 when we were both graduate students, I told him that someday someone was going to give a Turner-like paper at the AHA convention substituting ethnicity and religion for the frontier; personal communication from William G. Shade. The fashion now, of course, is to say that "ethnocultural" is being substituted for economic determinism—but Kelley's cultural interpretation resembles the Frontier hypothesis especially in the freight of conceptual ambiguity which it carries.

ignoring ethnicity, which does not constitute the whole of cultural interpretation but does form a large part of it. Cynthia H. Enloe faults her fellow political scientists for "historical shortsightedness" in underestimating ethnic factors in Western countries. This results, she says, from their beginning their studies at times when European nations had "subdued ethnic communalism and had consolidated political power." In non-Western countries nationalism came later and ethnic variables seem more critical. Scholars "had their analytical tools sharpened and ready for use when Burma and Kenya started to nationalize, whereas these tools were just being fashioned when Britain, Russia, and the United States were formed."²

Our intellectual heritage, moreover, has tended to treat ethnicity as an unwanted stepchild. Enlightenment rationalism and its ideological descendants, especially liberal democracy and Marxism, have been hostile to ethnicity. Theorists of each have often disposed of ethnicity either by diluting its meaning or by assuming it to be transitory and bound to disappear at a higher stage of development. As Enloe puts it, "Thomas Jefferson and John Dewey share with Lenin and Mao the dilemma of denying ethnic groups theoretically while being preoccupied with ethnic conflict strategically. Standing on a platform and pronouncing ethnic loyalty to be an anomaly will not make it disappear."³ Although democratic ideology has inhibited recognition of ethnicity's persistence, have not professional scholars, overwhelmingly middle class, assimilated, nationalist, and too often naively Olympian, felt the magnetism of melting-pot idealism more strongly than other groups? And of what Milton Gordon calls the "liberal expectancy" that ethnicity will someday wither away? Have not political historians, more than any others, written from assimilationist assumptions which have made their works part of the national effort to transcend ethnicity? Other inhibitors of a cultural approach include the materialism of the long-dominant progressive historians, and the implicit or vague economic determinism of so many other historians.

We need to be aware, however, of the cultural interpretation's sociology, and why it is finding acceptance in our time. I shall not consider this fully here, but it is particularly incumbent upon those to whom a cultural interpretation is most attractive to understand its origins. Certainly ethnicity is now in fashion. "Land, blood, and religion," to use Andrew M. Greeley's phrase, is all the rage, from pop culture and TV-land to seminars on the banks of the river Charles. From the latter Nathan Glazer and Daniel P. Moynihan, old hands at the melting pot, offer a new anthology breathlessly introducing ethnicity as, in its present form, something new under the sun. They announce that ethnic self-assertion and conflict have been greater in the world in the last twenty years than ever before, as if the Old Testament had never been written.⁴

² *Ethnic Conflict and Political Development* (Boston, 1973), 12.

³ *Ibid.*, 60; also see 80-81.

⁴ Greeley, *Why Can't They Be Like Us? Facts and Fallacies about Ethnic Group Differences and Group Conflict in America* (Old Bethpage, N.Y., 1969); and Glazer and Moynihan, eds., *Ethnicity: Theory and Experience* (Cambridge, Mass., 1975); 5-6. Also see Michael Novak, *The Rise of the Unmeltable Ethnics: Politics and Culture in the Seventies* (New York, 1971).

A sober approach to something so much with us might begin by recognizing that both ethnic awareness and ethnic politics are variables: they come and go and, while active, vary in intensity. Our time is emphatically one of ethnic consciousness although not, ironically, of ethnic politics to any exceptional degree. Its uniqueness derives perhaps from the thrust of ethnicity toward pride and pluralism and away from shame and assimilationist imperatives. A sense of ethnic identity waxes and wanes—one might say there is a threshold of awareness—and it varies in kind. It is above all subjective. It must exist not in the eye of the beholder but in the minds of its members.⁵

Kelley suggests this subjectivity in defining “cultural” to include what is normally meant by ethnic group as well as “home-grown ethnic groups like Yankees and white Southerners.” Kelley’s fluid and eclectic use of “ethnic” and “cultural” is both a strength and a weakness. Thus, as he moves through two hundred years Kelley discusses many groups at times when their “identity has been a salient problem.” One line of criticism here can only fault Kelley for not pursuing his own logic further and refining categories.⁶ These omissions are important, but forgivable in a paper of this length and scope. Yet one wonders if Kelley’s apparently casual approach to defining ethnicity has not caused him at times to overlook both its subjectivity and variability. Did “Roman Catholics” in the antebellum period identify as Catholics, or was religious identity for most bound up with their ethnicity, as it was with the Irish who were so central to nineteenth-century politics? Strictly speaking, Roman Catholics are not an ethnic group; and, if we adhere to the subjective basis of identity, it is doubtful that they qualify even under Kelley’s loose “cultural” rubric. Should we not think of even so strong a self-awareness as the Irish Catholics’—which Kelley assumes as constant—as varying over time?⁷ Disquiet arises, too, from Kelley’s tendency to freeze some ethnic groups into static postures to which they conform monolithically for one or two centuries. Aside from the fact that groups have assimilated and acculturated⁸ at greatly different rates, with consequences for partisan preference, this risks misrepresenting the many variations of ethnic awareness as they have existed here and throughout the world. The author of a recent survey of cultural plu-

⁵ Enloe, *Ethnic Conflict and Political Development*, 16. “In the final analysis, identity is a subjective, individual phenomenon; it is shaped through the constantly recurrent question to ego, ‘Who am I?’ with its inevitable corollary, ‘Who is he?’ Generalized to the collectivity, these become, ‘Who are we?’ and ‘Who are they?’ These questions involve the basic processes of cognition, perception, and symbol-formation . . .” Crawford Young, *The Politics of Cultural Pluralism* (Madison, 1976), 20.

⁶ To take only one of many possible examples: what about all the Yankee Baptists who were not Federalists but Jeffersonian Republicans? At what point did their Yankee identity become more salient than their Baptist (or other) persuasion, if it ever did? There is barely a hint that the Jeffersonians existed in New England. “Yankees” and “Jeffersonians” are not the only groups which Kelley treats without sufficient subtlety.

⁷ When Kelley delivered this paper in Washington, D. C., several members of the audience showed skepticism respecting his generalizations about the Scotch-Irish. Beneath these questions lay uncertainty about Scotch-Irish self-awareness and its durability and was, I believe, only partly related to lack of information about the group (which would itself tend to support the doubters). Still, for the periods involved I would wager that Kelley’s claims have general validity.

⁸ These are two distinct processes and do not necessarily occur together. Michael Parenti, “Ethnic Politics and the Persistence of Ethnic Identification,” *American Political Science Review*, 61 (1967): 717–25.

ralism in Asia, Africa, and Latin America remarked on the discovery by scholars in recent years of the "enormous complexity" of the communal factor, of "widely varying levels of intensity of collective awareness . . . multiple layers of self-consciousness . . ." With groups in constant flux, "any theory of ethnic conflict must incorporate change as a central element."⁹

AS ETHNIC AWARENESS CAN RISE AND FALL in response to political events, social change, and interaction with other groups, so too can ethnic and cultural politics be conceived of as variables. They sometimes dominate the political stage, and other times recede into the background. Historians have wondered about why certain periods provide an effusion of cultural politics, and we seem to have advanced little beyond the obvious wisdom that depressions remind citizens of economic issues, while certain "decades"—typically the 1920s—display together prosperity and cultural politics. Surely we can do better. Even if economic and cultural politics are sometimes inversely related, it is more common to find economic and cultural conditions or events reinforcing one another. Even when cultural issues seem completely to command public attention, there are consequences not only for "recognition," values, and status, but also in economic payoffs and in what Murray Edelman calls "the allocation of tangible resources."¹⁰ We can begin by recognizing that ethnic and cultural politics are variables, and by becoming sensitive to the interpenetration of economic and cultural politics and the too-often implicit social consequences of their interaction.

In contrast to Kelley's approach, ethnic politics can be differentiated from cultural politics, and different kinds of ethnic politics can be recognized. Yankee versus Southern confrontation in a national arena is a type of cultural politics different from Irish Catholic versus British Protestant ethnic conflict in a Northern town. Though they were related, pre-Civil War realignment politics within individual Northern states differed from sectionalization within the nation as a whole. Further, ethnic politics in 1750, 1850, and 1950 differ in kind, given changes in physical and social context, communication, and the mix of groups. Kelley does tend to homogenize, making the ethnic politics of the Jeffersonian era indistinguishable from those of the 1830s or 1930s. Indeed, I wonder if one can truly speak of ethnic politics in the early republic. After the 1830s his perspective works much better, and ethnic and even denominational politics do become central to political culture. Before then, however, the degree, context, and character of ethnic politics differed. Kelley has chosen to emphasize, and with good result, that our politics have

⁹ Young, *Politics of Cultural Pluralism*, 5. "The set of groupings which constitute plurality are not necessarily permanent, frozen collectivities, but in a state of flux in response to long-run forces of social change, shorter-run alterations in political context, and continuous processes of interaction with other groups . . . Each cultural aggregate may vary widely in the degree to which its identity pattern is given ideological formulation, ranging from highly developed theories of a group's collective history, cultural heritage, and common destiny to amorphous, ill-formed, and only barely manifest self-awareness" (pages 12–13).

¹⁰ *The Symbolic Uses of Politics* (Urbana, Ill., 1964).

possessed a continuous cultural dimension of some kind. The cost comes in losing a sense of variety and change.

Kelley's blurring of types of cultural politics leads him to miss a chance to attempt a true cultural explanation of the Civil War, of why North and South fought. Considerable confusion has surfaced recently about the contributions of the "new political history" to this question, and Kelley does not clarify matters. The recent literature on party loyalty and realignment in Northern states illuminates only one of many processes that need to be analyzed in any overall account of Civil War causation. The new political histories which describe voting have not explained (and have not pretended to explain) the motives and actions even of Northern leaders, only the context in which they acted. Much less have these studies offered to explain Southern sectionalism or secession. Influenced too much by recent scholarship, Kelley needs to rely more on the works of such scholars as Sydnor, Craven, Randall, Potter, Donald, Wooster, and others who have followed in areas Kelley has neglected. But however well he redresses this balance, he should recognize that Southern identity was not ethnic: from a territorial and economic base Southernness became a cultural difference and, among a significant minority, an aspiration to a national identity. In the North, cultural *and* ethnic politics played crucial roles in a cluster of causes which served to create a sectionalized Republican party. Historians have only begun to explore the ways in which these processes (among others) symbolically and politically interacted.¹¹

Kelley's omission of change bears heavily on how "political culture" fares in his paper. His thesis that our politics have been strongly shaped by cultural pluralism does a service in giving this point a weight of example and continuity. But the invocation of the increasingly popular term "political culture" creates expectations Kelley does not meet. Political culture generally refers to the aggregate political life-style of a nation, state, or region, and to orientations, to formal and informal rules, to values, etc., that are pretty much taken for granted. The term may be used to refer to specific groups, and Kelley does in fact describe the behavior of various political *subcultures* from George III to Carter. But the sum of parts does not automatically render the whole; Kelley does not pause to generalize about political culture overall.¹²

If this omission is partly a consequence of what Kelley chooses to empha-

¹¹ Joel H. Silbey, "Social Conflict and the Coming of the American Civil War: The Perspective of the New Political History," paper delivered at the annual meeting of the American Historical Association, December 30, 1976 in Washington, D. C. Silbey discusses the Southern Democratic image of Republicans as sectional and cultural imperialists with reference to the resonances shared by Northern and Southern Democrats.

¹² There is consensus, for example, that from the 1780s to the 1840s political culture changed from a deferential, traditional politics to one dominated by egalitarian styles and mass parties. Ronald P. Formisano, "Deferential-Participant Politics: The Early Republic's Political Culture, 1789-1840," *American Political Science Review*, 68 (1974): 473-87. This type of global change needs to be connected to the elements of the causes of the Civil War discussed above. For example, after viewing the threats of violence expressed during the Missouri Controversy of 1819-21 by both Northern and, especially, Southern Congressmen, Glover Moore guessed that, if civil war had broken out in 1820, it would have rattled only through the chambers of Congress and not involved the population. *The Missouri Controversy, 1819-1821* (Lexington, Ky., 1966), 175. Kelley's neglect of national political culture does not really move us across the four decades and to the conditions producing a civil war of peoples.

size, his neglect of community context constricts his understanding of cultural pluralism's variety and its interactions with social structure and political events. Kelley knows that an ethnic group may change its role over time (for example, from outsider to insider). But I am still disturbed by his comments on the "great age and durability of the party system," and his assertion that "two clusters of peoples and ideas coalesced against each other in the Revolutionary generation, and with slow modifications and accretions they appear to have endured to the present day." In contrast, Kelley himself observes that the parties have shifted positions on state paternalism and moral activism. Although outsiders have tended to cluster together against insiders, the objection is not just that the mix of groups has changed more than Kelley acknowledges. For at any given time some ethnic groups might play a dominant or subordinate role depending on the community context. Thus a cross-sectional view of cultural pluralism in a continental nation of infinite heterogeneity must be prepared to find groups in different mixtures and roles.

One brief example of community context will serve to illustrate its importance. In studying voting patterns in Michigan during the antebellum period I guessed that, before the 1850s, native Methodists—something of an outgroup in the New England diaspora—reacted to a strong Yankee Presbyterian elite group in the Whig Party and divided in their party choice, with a Democratic tendency. In a recent dissertation on Greene County, Illinois, John Michael Rozett has employed poll books recording individual votes to study the social bases of parties. Rozett found that, unlike the Michigan Methodists, seventy-four percent of the Methodists in Greene County were Whigs. But place of origin was important. Thus all of the foreign-born Methodists, who were mostly English, voted Whig. And this small English contingent possessed visibility and influence far beyond its numbers. The English arrived with the county's first settlers, helped build the Methodist churches, and vied for political leadership with pioneers from the South (who tended to be Democratic). These foreign-born Protestants, who might superficially be regarded as immigrant outsiders, were a high status, active group. They set off reactions much like the Yankee Presbyterians in Michigan and helped to create the constellation of subcultures in Greene County.¹³ In calling attention to the variety of political alignments which historical and social context may create, Paul Kleppner has remarked that "it is upsetting to the . . . 'grand synthesis' to discover . . . that the same specified set of social attributes produced different partisan identifications in distinctive contexts."¹⁴

¹³ I have used only a tiny part of Rozett's findings for illustration here: "The Social Bases of Party Conflict in the Age of Jackson: Individual Voting Behavior in Greene County, Illinois, 1838–1848" (Ph.D. dissertation, University of Michigan, 1974), 165–66, *passim*. This example reminds us that Kelley gives too little sense of the fact that "ethnics" often voted for the core culture party (e.g., British, Scandinavians, *et al.*)

¹⁴ Kleppner, "Parties, Voters, and Political Culture: The Third Party System, 1853–1892," draft of forthcoming manuscript, ch. 9, 3. Kleppner puts it succinctly: "Any social attribute may have different meaning and effect on different people depending upon historical and contextual conditions" (page 7). Because the "impact of ethnic and religious identification was no more contextually invariable for native stock groups than it was for 'adopted citizens,'" Kleppner argues that research designs must permit "contextual explorations" (pages 10, 15).

In an open and mobile society cultural pluralism has been associated with class stratification and inequality among and within cultural groups. Those left out have often expressed themselves in other than a cultural idiom. Protesters, challengers, and radicals are wholly ignored in Kelley's essay, but the margins can tell us much about the center. In addition, there is the connection between nativism and right-wing antiradicalism, which forms part of the story of the failure of class politics, especially in this century. Cultural interpretations should also help explain why a popular class politics has been relatively blunted. And the effects of ethnic and cultural politics on social justice and equity might be assessed without making ethnicity a scapegoat—though the irony of such a turn of events must seem almost irresistible to Clio's often cruel sense of humor.

If ethnicity should attract blame for blocking "Progress" in many spheres, it will be through no fault of Kelley's because he, at least, does not assume that assimilation is necessarily good or natural: there is no liberally expectant hand-wringing here over the persistence of dark and irrational forces. Kelley does not therefore assume, as so many do, that ethnicity is socially dysfunctional. Its functions or dysfunctions are matters of inquiry, not of assumption.¹⁵ While Kelley avoids an assimilationist bias, I wonder if there is not a different kind of optimism present. Is there in Kelley's framework an invisible hand bringing a common good out of cultural conflict? Perhaps not. But Kelley exaggerates the degree to which governments and parties have actually represented ethnic groups. Explaining elections in terms of coalitions is not the same as explaining governmental policies. Because most of those voting in a particular group are loyal to one party does not mean that their needs are met, their hopes realized, or even—as more often does happen—their fears of encroachment allayed.

I take less satisfaction in the ongoing "party" than does Kelley. He speaks of groups in recent elections voting "overwhelmingly" for a party, but in the 1970s masses of eligible voters have not gone to the polls at all. A significant part of the electorate previously interested in politics has become "disenchanted."¹⁶ Although citizens appear to pay more attention to politics, to vote more with an eye to issues, and to think increasingly that what happens in Washington affects their lives, nevertheless many have simply tuned out. And this trend began in the 1960s, fostered by Vietnam, before Watergate.

¹⁵ Enloe, *Ethnic Conflict and Political Development*, 33. Enloe also separates modernization from development, and development from ethnicity. She says that the puzzle of "the surprising vitality of ethnicity in advanced countries" might be explained by positing that "development and ethnicity are not inversely related" as so many social scientists assume: "They may not be related at all" (page 268). Enloe's conclusions are must reading on this subject; see pages 261–74.

¹⁶ Thus any statements about the "ethnic vote" based on data showing, for example, a 60 to 40 percent division in a particular group should be qualified by recognizing that the group generalized about excludes the 40 percent otherwise and the non-voters. Martin Plax, "Towards a Redefinition of Ethnic Politics," *Ethnicity*, 3 (1976): 23–24. Plax forcibly suggests too "that any work that deals with the effects of ethnicity must be able to define the boundaries of that effect as well, so as to make it possible to reveal when some behaviors are *not* related to a person's ethnicity, even if many nominal ethnics act in the same way" (page 25). Plax' trenchant critique of writings on ethnic politics has less force for the nineteenth century when voting turnout was much higher and ethnic consciousness greater.

James Young described the federal establishment of the early nineteenth century as "Government at a Distance and out of Sight." In the 1970s the government is well in sight, at least those ceremonial and dramatic aspects on which the media focus, but it is perhaps more distant than ever. Trust in government and politicians has declined precipitously, and Walter Dean Burnham has tied these trends to the long-run decomposition of parties in the electorate during this century. Party loyalty survives, but probably at its lowest level since mass parties formed. Alienation and detachment among both old and new voters is such that the authors of *The Changing American Voter* not only question "the responsiveness of the political process" but actually wonder, "Is the United States becoming ungovernable?" Like Burnham, Nelson Polsby has pointed to the 1890s as a turning point for representative democracy by demonstrating a dramatic contrast between high levels of turnover among Congressmen in the nineteenth century as against low turnover and high incumbency in this century. Indeed, in the last election 96.7 percent of incumbents were returned. In this situation one is prompted to ask who represents whom?¹⁷

IN CONCLUDING THESE COMMENTS I must note that Kelley has already shown a cheerful capacity to absorb critical advice. Therefore, one turns to the concluding question—what needs to be done?—with more than ritual energy. Inquiry—for Kelley and others—should proceed from the premise of cultural pluralism in two opposite but complementary directions: 1.) the comparative and cross-national (as a few historians including Kelley and some political scientists have already done); and 2.) internally, with intensive studies of communities that would address questions raised above.

We need to be precise in defining what we mean by "ethnicity," "ideology," and "political culture," while recognizing that these concepts are as complex and slippery as any to be found in social science. The use of each can be aided by making distinctions explicit. Enloe, for example, usefully distinguishes three kinds of ethnicity: tribal, national, and racial.¹⁸ Kelley seems to be concerned mostly with the second of these. Indeed, he reveals his idea of "nationality-ethnicity" by reference to "those whose ethnicity is of foreign origin." This reflects the popular assumption that all ethnic groups transported a national identity intact across the ocean. But Rudolf Vecoli, Andrew M. Greeley, Joshua Fishman, and others have tried in recent years to correct this view. A sense of nationality often came into being largely on these shores. Although immigrants did not arrive *tabulae rasae*, those who came

¹⁷ Norman H. Nie, Sidney Verba, and John R. Petrocik, *The Changing American Voter* (Cambridge, Mass., 1976), 2; Nelson W. Polsby, "The Institutionalization of the House of Representatives," *American Political Science Review*, 68 (1968): 144–68; and the works of Burnham found in Kelley's notes.

¹⁸ Enloe, *Ethnic Conflict and Political Development*, 23. This trio is given only to be suggestive; see, for example, Young's comments on the revisionist criticism of "tribalism," *Politics of Cultural Pluralism*, 34–37.

especially during the period 1880 to 1920 brought little of the self-conscious nationalism of their native lands:

The slogans, the anthems, the banners, the crusades, the poets, the saints—the entire fiery sword and flaming ideology of political and cultural nationalism—apparently made little, if any, permanent impression on millions who left Europe to come to the United States. . . . Ethnicity of a traditional, particularistic, and non-ideological character—rather than nationalism in its strident and symbolically elaborated manifestation—was the general rule. The languages spoken were related to the countless acts of everyday life rather than to “causes” or ideologies. Most usually it was only *after* immigration that group maintenance became a conscious goal. In some instances, indeed, it was only in America that many immigrants became aware of their “groupness,” i.e., of their common origin and common past¹⁹

By the same logic, it is not useful to think of ethnic identity as “primordial.” Rather, historians must explore far more than they already have the formation of ethnic identities, their social psychologies, and their political subcultures.

We must be careful, too, with the term “ideology” and whom we accuse or compliment for having one. There is little in Kelley’s paper about “ideology” according to most definitions of the word. There is a good deal about the values and voting of subcultures, and of politicians’ and parties’ symbolic politics. We are not given discussions of white Southern, Scotch-Irish, Italian, or Scandinavian ideologies. If by ideology we mean a belief system having abstract objects of centrality, wide scope, and constraint, then it is difficult to grant that mass publics have ideologies. On the other hand, we can surely speak of more loosely organized systems of political belief for ethnic and other groups. But this is a complex matter and becoming more so. Karl Mannheim, who had not a little to do with creating our notions of “ideology,” wrote that “during the Renaissance, among the fellow citizens of Machiavelli, there arose a new adage calling attention to a common observation of the time—namely, that the thought of the palace is one thing, and that of the public square is another.”²⁰ Since Watergate this conviction has been compelling among virtually all classes of citizens. Meanwhile, political scientists have debated strenuously whether or not the mass public shows ideological consistency in its beliefs and party choice. All have tended to agree that since the mid-1960s some of the electorate has paid more attention to issues and has aligned itself more logically in terms of ideology, though the meaning of this is still in dispute. We have not, however, reached a situation where differences have disappeared between thinkers in the palace and those in the public square. We need to understand better the different forms of ideology and belief as well as their congruences.

¹⁹ Joshua A. Fishman, ed., *Language Loyalty in the United States: The Maintenance and Perpetuation of Non-English Mother Tongues by American Ethnic and Religious Groups* (The Hague, 1966), 27. The process for one group is brilliantly described in Richard Gambino, *Blood of My Blood: The Dilemma of Italian Americans* (New York, 1975). Also see Greeley, *Why Can't They Be Like Us?*, and Oscar Handlin, *Boston's Immigrants* (Cambridge, Mass., 1941).

²⁰ *Ideology and Utopia* (New York, 1936), 63.

Finally, why should these things be done? Because historians can contribute to theories about the relationship between class and culture, can particularly explain why a popular class politics has failed to develop, and can suggest the role of cultural pluralism in that failure. In addition, the decline of melting-pot transcendentalism has left a void which cultural pluralism only partly fills. The assimilationist model is both descriptive and prescriptive—cultural pluralism is largely the former; it remains vague about national goals and about just how ethnic pride helps us all to be “better Americans.” This need not be the case. Surely there is more to hope for than a choice between separatist romantic *volkism* and conformist idealistic nationalism. We live in a time of both ideological conviction and ethnic awareness, but the relationship between the two impulses is not clear to us. Our time may be unusual in the intensity and apparent separateness of both. Or both may share their origin in the quest of citizens to regain efficacy and meaning in their lives. In the past ethnic and cultural politics have seldom gone much beyond the symbolic to address the material sources of popular discontent. Without invoking the spectre of “false consciousness,” and with due respect for peoples’ values and circumstances, it may be possible for historians to help give citizens more than a *sense* of regaining control and purpose in their lives.

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THIS IS AN IMPORTANT PAPER, one few historians could have conceived in all its parts. Just assembling this splendid and up-to-date bibliography in political history would have been a notable contribution for Robert Kelley to have made, but to have this excellent and thought-provoking effort to synthesize American political history along lines of ethnic and cultural conflict makes this a special occasion. Readers of *The Transatlantic Persuasion*, with its bold and stimulating comparison of American Democrats with their English and Canadian counterparts, would expect no less of its author. That excellent work was broad in every sense, while the present study promises to be long as well as broad. This observation brings up a cause for difficulty in raising objections to the discrete parts of Kelley’s very strong paper. Conscience

makes cowards of us all, and I know that what appears here is in some ways merely the skeleton of a book to be called *Bred in the Bone: The Cultural Pattern of American Politics*. It seems churlish to pick flaws with Kelley on points that will certainly be treated more elaborately in the forthcoming book. His plan to trace the connection between grass-roots culture and the arena of political action over and through five “party systems” deserves high praise, for it is just those connecting linkages at points of change that have been missing or weak in much of the ethnocultural analysis of politics that we have previously seen. Explaining *why* is considerably more difficult than showing *what* happened at the polls in a given election. A statistical correlation may suggest more than one reason for a given result.

But I am sure that Robert Kelley is not sharing his work with us for applause, or even encouragement alone, but for suggestions as well. I will be brief and suggest only three points on which it seems to me the success of this overarching interpretation will be likely to rest. I think, in the first place, that he will want to make a little clearer in his book than he has done here the precise mechanics of how cultural attitudes are translated into political power and influence. Secondly, is it not possible to incorporate a reasonable amount of enlightened self-interest in the motives of voters? After all, if everyone is in some kind of ethnic or cultural grouping (as is postulated in this paper), everyone is implicitly motivated more by ethnic prejudices and beliefs than by anything else. Then there would seem to be no voters—or very few—who know or care whether a given economic program affects their interests. Were there so few who understood national issues? How can a predominantly ethnocultural analysis be blended with other factors that must be acknowledged and still retain the lines of the ethnocultural pattern of politics without taking refuge in mere assertions of what the *real* motives were? How does one single out the critical force when two or more forces converge to the same result? And, in the third place, a successful thesis of United States political history must account in a satisfactory way for the coming of the Civil War. This observation owes something to the fact that the ethnoculturalists have been much engaged in analyzing the dissolution of the second party system; and the convergence at that time, on the eve of the Civil War, of the classic issues of the expansion of slavery and the great Irish Catholic immigration makes the 1850s something of a testing ground for any ethnocultural explanation of the American past.

In some respects I prefer the descriptive subtitle Kelley has chosen for his forthcoming book to the title of the paper just presented, because I can understand more clearly what “the cultural pattern in American politics” means than what the less delimiting expression “ideology and political culture” denotes. I regard “cultural pattern” as a relatively explicit disavowal of uniqueness in explanation, but Kelley’s intention as expressed in the second title—the one he employs here—raises questions about definition of ideology and culture and about the relationship between them that he wants to establish. From much of the phrasing and the general organization of the

paper I have the idea that more than “pattern” is intended. In that case, all the parts are elements in a linked argument, and many targets are open to throwers of small stones:

Was the British Empire in a real sense either a political or cultural *community* on the eve of the Revolution? Its disintegration would almost argue against that. For how long are the Scotch-Irish recognizable as the “dour Scots” Kelley mentions? They went into the melting pot very early, and one questions the forthright authority with which Kelley advances the idea that they were sufficiently self-identifying and identifiable to produce the “landmark shift” in the lower North from the Democrats to the Republicans in the election of 1860. And *did* they change their allegiance (*if* they did) because they hated the Catholics as new members in the Democratic club so much? Or was it perhaps because they hated the Slave Power Conspiracy and took note that Southern slaveholders were Democrats? How do we know that the New Englanders really regarded the national government under the constitution as divinely appointed in the sense that their town governments had been? The acid test would come when the government went against their interests, and we consider how they responded. In this regard we need to recall that the Southern states were not the first to register an impulse to secede.

There are some white Southerners who will be relieved to be elevated to the status of an ethnic group, for they may reflect that this classification may spare them some criticism. “Redneck” and “Cracker” are among the few remaining social slurs of caste or class now appearing in respectable publications, and admission to ethnic status might change that! And yet describing this group as a cultural entity requires some fancy footwork and a special regard—or disregard—for time and place. I can hardly bring myself to view the Whigs as an “outgroup” in a South that they tended to dominate culturally and socially. A very respectable portion of the political and financial leadership tended to be Whig in most states until the War loomed. The South has hardly been a monolith (even on the edge of the Civil War), and though it has shown remarkable cohesion at some critical points in our national history, there have been internal differences and great regional distinctions. There have also been cross-sectional political alliances of considerable potency. Kelley states that the white Southerners were “libertarian Republicans” because they wanted “above all” to be free to own slave property; but only a few pages further he says that they looked for markets in the West because they hoped to throw off slavery through a diversified economy. Well, perhaps they were “passionate localists” merely because they were an agricultural people who lived further away from one another than farmer folk in the North did, because they then lived on isolated plantations and visited the larger world—if it might be called that—once a month, on Court Monday, when they took a horseback or buggy ride to the county seat. One could go on thumbing through these details, without knowing whether these arguments may already have been met in the book, and all contradictions resolved.

More attention to the pure mechanics of how the cultural impact is expressed in political ideas might clear up much of this type of confusion. *How* are the prejudices, hopes, and identifications of the voters assembled by the political leadership and directed toward the goals of a given party? Through slogans, surely—powerful symbols that evoke clusters of impulses and manipulate them. Walter Lippman has said that symbols become powerful politically when they lose their specific meaning and gain a generalized emotional content. Using that concept Chaplain Morrison studied the Wilmot Proviso and demonstrated just how the Proviso expressed and organized (emotionally speaking) the differences between North and South over the issue of slavery in the territories. The Proviso assumed the character of a slogan and became the succinct statement of the Republican position in the sectional conflict; but, ironically enough, it was first articulated by a Democrat for (as it seems) the purposes of internal party politics—to measure the Southern Democrats' professions of party loyalty; to expose them, so to speak, for President Polk to see; to test whether they were deserving of the favoritism Northern Democrats believed the President to be showing Southern leaders of the party. This example indirectly raises a question of whether the political elite may not, sometimes, in assembling and organizing cultural impulses from the grass roots, be just as effectively *creating* those cultural impulses. What are cultural issues anyway? Kelley refers to the Communist scare of the early 1950s as being "deeply and fundamentally cultural." One can certainly agree with that, but there is a question of whether the political leaders were manipulating an issue or creating one. Are there not many instances similar to this one? Kelley assigns a cultural character to the slavery issue, which it surely had. But it also had an economic, a sectional, and a psychological character.

The history of leading classes and of political figures has been attacked for elitism, but much of it seems remarkably democratic in its basic assumptions concerning the ability of ordinary voters when compared with much of the ethnocultural work appearing recently. Are our historians being misled by patterns of ethnic voting that correlate with religious affiliations into thinking that these correlations also provide *reasons* for voting? Are we not in danger of substituting a new kind of determinism—cultural—for the old—economic or socioeconomic determinism? Are we now to see the ordinary voter as helpless in the grip of his ethnic convictions or prejudices as Beard's voters were impelled by their economic interests? It is sad to find so pitiable a picture of the grass-roots voter, when we view history from the bottom up.

No general interpretation of our record will be convincing if it fails to account in a satisfactory way for the Civil War. After all, this was the point when all systems broke down, and it must be accounted the most significant of our failures and the most important of our triumphs. My distinguished colleague and fellow commentator, Ronald Formisano, once referred, quite aptly I believe, to the causation of the Civil War as the Sargasso Sea of American historiography. I agree that it is an entangling morass and that most of the scholars who have tried to cut boldly through it have had only

temporary success. The facts and the variables have proved impossible to account for under any single theory up until now. Charles Beard's famous explanation was the first idea about this conflict that many of us learned: the expanding industrializing North grew increasingly restless at the constraints of a political economy dominated by the dead hand of an archaic Southern leadership based on cotton plantations and slavery, and the North's throwing off of these constraints resulted in secession and war. I suppose no sensible person would even today deny some part to economic drives in accounting for the conflict, but the discovery that Northern business interests that were supposed by this explanation to have profited from the war could not agree on what good things, economically speaking, should derive from it has damaged Beard's thesis beyond redemption in the minds of many scholars. Perhaps more subtly disturbing was the implicit contradiction that seemed to appear when the proposition was examined closely: group conspiracy was combined with economic determinism, willful action with historical necessity. To conceive of conspiracies as inevitable seems perverse and in time this aspect of Beard's thesis fell hardest of all.

The revisionists denied the inevitability of Beard's argument, and attributed the conflict to inept and shortsighted leaders who were serving their own ambitions. Knowing that the conspirators could not logically agree, it is entirely possible for conspirators to disagree once they have achieved victory. As all men know, the revisionists in our time have been revised, and we have learned to regard the conflict as just possibly having been inevitable because it was all about the moral issue of slavery. Now we have another challenge in the ethnocultural group of historians who discount slavery as being very significant, as either a moral or an economic fact, in the coming of the Civil War. It hardly seems too much, considering the difficulties of explaining all of this to an increasingly skeptical student body, to demand of any new sweeping interpretation of the nation's political history that it give a serviceable account of the causes of the Civil War and that, if that causation should turn out to be monolithic, the monolith be convincing indeed.

Is Kelley's interpretation of the Civil War too monolithic? To the extent that he leans heavily on the findings of the ethnocultural cliometricians of our guild, I suggest that he must check the several parts of his study carefully. Most of the work done to date has been concentrated on Northern localities, states, cities, and regions, and has explained, on the whole, that the issue of slavery's extension was not very significant in the minds of the voters when compared with the nativist impulse. The assumption here is that the success of the new Republican party—which owed indeed a great deal to the cultural impulses of nativism, temperance, evangelicalism, and so on—was the precipitating cause of the Civil War. Whether this is a right idea or a wrong idea I will pass by for now, beyond mentioning that the Know-Nothing party did itself eventually dissolve over the issue of slavery. The rise of the Republican party would not have in itself “caused” the war if the South had not seceded. That was the precipitating factor. The sense of being encircled *with* slavery

and *with* the issue of race no doubt predated the Republican victory of 1860. I tend to think no theory based primarily on cultural voting patterns in the North is going to explain the coming of the Civil War.

To sum up the central suggestions I wished to make: 1.) I hope Kelley will find a way to make clearer the precise way in which the cultural impulses he traces over time become effective or salient politically at a given time. An example would be explaining *how* or *why* the Democrats become the champions of black rights and activists as moral crusaders in contradistinction to their earlier hands-off attitude. Why *now* do Democrats think we must *not* be free to enact our low prejudices? 2.) Then we need to know something more about these voters beyond their inclinations to vote with others of their national or religious affiliation and against those whom they traditionally despise. These identifications are easier to measure, and because of that it becomes easy to mistake an effect for a cause. This was an age of high voter participation, and from all I can see the voters were well informed. The voters read newspapers and were not vitiated by television. Surely the politicians were not endlessly speaking and writing about issues for their own elite pleasure. They had a target. I suppose most of these voters did understand their own interests, economically and socially speaking, and that these interests not infrequently coincided with ethnic identification. 3.) To continue in the belief that slavery was at bottom the cause of the Civil War is not to be deceived about the motives of those who voted with the Republicans to contain slavery within the bounds of the then slave states. One did not have to be an egalitarian or even very friendly to black people to want slavery put on the road to extinction. The essence of the Free-Soil movement seems to be hostility to all the trappings of the slave power—the master, the plantation, the slaves, to the black people, free or slave. The presence of slavery corrupted and endangered the freedom of white farmers, an idea that broke up the nativist American party just as it had the Whigs, an idea that divided the Democrats at the last, in Charleston.

These suggestions and objections notwithstanding, this is an important study, a humane piece of historical writing. It has blood in it, and people too, though they *are* a mass. But the success of the work is best recognizable when Kelley charts his course at a safe distance from the treacherous shores of the new determinism that the ethnoculturalists have located through a narrow examination of voter behavior.

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The Aging of America

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THE GREAT AMERICAN BIRTHDAY PARTY OF 1976 is over. Visiting captains and kings have long since departed. A few belated revelers persist with scheduled ceremonials, but the main celebration is behind us and the anniversary year has ended. The time has come—as it inevitably does for all those of advanced years who join in the celebration of their own birthdays—to admit of a certain ambiguity of feelings about what exactly was being celebrated. On such occasions survival surely justifies some self-congratulation—especially when one considers the alternative. The fact remains, however, that all existence is finite. And the question naturally arises, how much celebration and how many congratulations are acceptable over the approach of the inevitable. As Paul Valéry observed in the seventh decade of his life, “The future is like everything else, no longer what it used to be.”¹

Regardless of what Americans thought they were celebrating with such enthusiasm and expense and international cooperation, it is obvious that the chief significance of the occasion lay in the passing of a dramatically advanced milestone in the aging of their nation—an advanced age even for nations. I shall return later to the relativity of age among nations and promise to make due apologies for the analogies between human and national life and lifespan. In the meantime, however, I shall perversely continue to employ the analogies for limited and, I hope, valid purposes. The point about aging and the consciousness of it is introduced at this juncture to emphasize the contrast between the reality and the typical way in which Americans did in fact celebrate their bicentennial anniversary and the symbols they selected for what they found significant for them in the occasion.

What they spontaneously and almost universally preferred and doted upon were not the symbols of age and maturity, but rather the symbols of youth and innocence. Thousands of Americans marched in eighteenth-century uniforms and re-enacted youthful rebellions against parental authority. They paraded in quaint costumes and displayed antique weapons, implements, and vehicles. They made pilgrimages to the birthplace and the cradles of the republic—to Concord, Philadelphia, and Williamsburg. They admired eigh-

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¹ Jackson Mathews, ed., *The Collected Works of Paul Valéry* (New York, 1962), 10: 171.

teenth-century heroes and Founding Fathers. National news magazines printed colonial portraits on the covers, and one weekly published whole issues dated July 4, 1776 and September 26, 1789, reporting events of those weeks in slightly antiqued typography as if they had just happened. Sympathetic nations, and some not so sympathetic, took their cue from American whim and fantasy: from around the world governments dispatched old sailing vessels (with supplementary modern power) to American harbors. Suddenly and mysteriously the so-called tall ships became the prime symbols of whatever it was Americans yearned for, and they swarmed to the harbors, shores, and river banks by the millions in adoration.

It was all so different from the centennial celebration of 1876, where the most modern machines and technology were displayed at the Philadelphia Exposition as favored symbols. But the national mood of 1976 could hardly be described as nostalgic. Nostalgia was the happily inspired theme of the American exhibit of the International Expo at Montreal in 1967, filled with old movies, old hats, old clothes that people actually remembered. (It was the Russians who filled their exhibit at Montreal with the latest machines and technology like the Americans had a century before.) Memory and nostalgia, however, were not the appeal in 1976. It was something else, something quite different, some obscure and unconscious national impulse and need.

One recalls in this connection a remark of D. H. Lawrence about James Fenimore Cooper's Leatherstocking novels, that "they go backwards, from old age to golden youth. That is the true myth of America. She starts old, old, wrinkled and writhing in an old skin. And there is a gradual sloughing of the old skin, toward a new youth." And he repeats, "It is the myth of America."² Lawrence is not speaking of the reality, of course, but of the myth. There is a great deal in the record, both domestic and foreign, to support his theory of an America perceived as the land of youth, perennially seeking renewal, a new greening, the eternal Peter Pan among nations. So conceived, America is the land of beginnings, the country of the future, the place where things happen first, a nation of great expectations, and most of all the country of the young.

Among Americans Ralph Waldo Emerson, in his lecture "The Young American" in 1844, voiced the myth as well as any: "I call upon you, young men, be admonished, to obey your heart and be the nobility of this land. . . . Who should lead the leaders, but the young American. . . . free, healthful, strong. . . . It is the country of the Future. . . . a country of beginnings, of projects, of designs, of expectations."³ He could return to the same theme twenty years later toward the end of the most terrible war in American history: "Everything on this side of the water inspires large and prospective action," he wrote in 1864. "America means opportunity, freedom, power. . . . when we are reproached with vapouring by people of small home territory, like the English, I often think that ours is only the gait and bearing of a tall

² *Studies in Classic American Literature* (New York, 1951), 64.

³ "The Young American," in F. I. Carpenter, ed., *Ralph Waldo Emerson: Representative Selections* . . . (New York, 1934), 165.

boy, a little too large for his trousers, by the side of small boys.”⁴ How “Uncle Sam” ever came to be represented as white-haired and white-bearded is a mystery wrapped in a paradox.

The archetypal hero of American literature is disclosed by R. W. B. Lewis’s study, *The American Adam*, in the youthful innocence of Herman Melville’s Redburn, his *Pierre*, and his *Billy Budd*, in Mark Twain’s *Huck Finn*, in William James’ *Daisy Miller* and his *Isabel Archer*, and later in tormented distortions of the Adamic prototypes such as F. Scott Fitzgerald’s *Jay Gatsby*.⁵ Kenneth S. Lynn has stressed “the psychic immaturity that inspired so many childish themes, childish characters, childish points of view” among classic American writers of the nineteenth and twentieth centuries. He ascribes this “immaturity of American literature,” this “natural enthusiasm for avoiding problems rather than confronting them,” to what he calls “the historical circumstances that fostered childishness in an entire civilization.”⁶ In *The Machine in the Garden*, Leo Marx subtly traces American yearnings for the pastoral ideal, the simple rural paradise, in conflict with persistent claims of industrialization and urban life.⁷ Henry Nash Smith in his classic *Virgin Land* demonstrated how the myth of regeneration and renewal embodied in the symbol of the garden inspired not only writers and artists but farmers and immigrants, politicians and public policy, a whole society.⁸ The insight shared by these scholars is the vision of a society eternally dedicated to innocence and youth and in some measure resisting maturity and denying age.

Foreign observers of America have often noted manifestations of the myth and sometimes shared its illusions. They include some who knew us best. Their testimony began quite early in our history when there was obviously more reason for identifying Americans with youth, but one does not have to go back to the seventeenth or eighteenth or even the nineteenth century for suitable examples. Here is George Santayana writing in 1920: “What sense is there in this feeling, which we all have, that the American is young?” he asked, doubtless thinking of his years of American residence. “His country is blessed with as many elderly people as any other, and his descent from Adam, or from the Darwinian rival of Adam, cannot be shorter than that of his European cousins. Nor are his ideas always very fresh. . . . In spite of what is old-fashioned in his more general ideas, the American is unmistakably young. . . . I am not sorry to have known him in his youth. The charm of youth . . . lies in nearness to the impulses of nature. . . . Even under the inevitable crust of age the soul remains young, and wherever it is able to break through, sprouts into something green and tender. We are all as young at heart as the most youthful American, but the seed in his case has fallen upon virgin

⁴ E. W. Emerson and W. E. Forbes, eds., *Journals of Ralph Waldo Emerson with Annotations* (Boston, 1914), 10: 84.

⁵ R. W. B. Lewis, *The American Adam* (Chicago, 1959), 128–129.

⁶ “Adulthood in American Literature,” *Daedalus*, 105, #4 (1976): 49–59.

⁷ New York, 1964.

⁸ New York, 1950.

soil. . . .”⁹ It was the eternal greening of America again, and again the Virgin Land.

BUT NOW IT IS MORE THAN HALF A CENTURY LATER. Having just celebrated our nation’s two-hundredth birthday, we might reasonably be expected to be somewhat more aware of “the inevitable crust of age” than we are of those “green and tender” sprouts. And yet through the crust of centuries the greening impulses still show. “Nations, like individuals,” as I have said in another connection, “are slow in adjusting their mental habits to the process of aging. Patterns of thought, expectation, and self-image are normally fixed in youth and tend to persist past the age to which they are appropriate.”¹⁰ America would seem to have had more than its share of such problems, and some of them are still with her.

Perhaps this is the place to pause and admit that there are limits to the usefulness of any analogy between human and national life, between the biological and the institutional. There is no normal lifespan for nations, no foreordained cycle from birth through growth, decline, and death. Whatever use we make of this comparative frame of reference must therefore be figurative. With that acknowledgement, however, it is still possible to speak of the “aging” of nations and to think of their conduct in terms of their comparative age.

If we overlook the murky national origins of some oriental states, the one hundred and forty-odd more or less “sovereign” nations that at present hold seats in the United Nations General Assembly, along with the handful that are excluded, can be said to represent six fairly distinct generations of nation states. Of these the United States is the first born and considerably the oldest member of the second generation, which consists of offspring of the small family of European parent nations that made up the first generation of nations in the Western world. Their offspring in the New World spent their infancy in various stages of colonial dependency to “mother” countries. As “the first new nation,” the United States achieved national independence so far in advance of her siblings to the south and north, nations which came along in the following century, as to constitute almost a separate generation by itself. Since then four more generations of new nations, three small ones and one very large, have successively made their appearance. A small third generation was born in central and southern Europe with the wave of romantic nationalism in the mid-nineteenth century, a fourth later in the Balkans out of a revolt against the Turks, and a fifth in Eastern Europe as an aftermath of the First World War. The sixth, and by far the largest, emerged in Africa, Asia, the Middle East, and the Caribbean in recent years largely as a consequence of the decolonization of European holdings in those parts of the world.

⁹ *Character and Opinion in the United States* (New York, 1956), 111–12.

¹⁰ C. Vann Woodward, ed., *The Comparative Approach to American History* (New York, 1968), 346.

Order of birth and generational precedence are not the only ways of thinking of relative age among nations. Spain is one of the oldest among the first generations of Western nations. Yet this did not prevent King Juan Carlos I from telling a joint session of Congress in Washington last spring, "Spain today is a young and renewed nation. Two-thirds of us are under 40 years of age," said the thirty-eight year old monarch. "We are an old race, but at the same time a new people." A less superficial criterion than geriatric population is the continuity and age of the constitutional order of nations. Most nations, some of the oldest as well as many of the youngest, have undergone revolutions, sometimes successive revolutions, that have fundamentally changed their form of government and basic institutions. In this respect, American history has been singular. In terms of continuous constitutional existence without revolutionary disruption, the United States has the oldest, but one, of all existing governments—including those of the original parent nations.

By more than one criterion of relative antiquity, America can therefore be represented as standing in United Nations Plaza surrounded for the most part by puling infant nations, many of them with oedipal problems about the "mother" country and most of them still in their constitutional diapers. Yet both at home and abroad the images of American youth and immaturity have persisted over the centuries. America is always said to be coming of age, or about to. As long ago as the seventeenth century, James Harrington prophesied that the American colonies would demand their independence "when they come of age." Two other Englishmen, Richard Price and Thomas Paine, both friends of the American cause in 1776, invoked the metaphor in eighteenth-century prophesies.¹¹ Prophets continued to labor the same "coming-of-age" figure of speech right down to our own times. In 1915, Van Wyck Brooks announced it in his *America's Coming of Age*; in 1920, Santayana speculated that the American "is perhaps now reaching his majority, and all I say may hardly apply at all tomorrow"; in 1927, Andre Siegfried seemed to confirm the prophecy in his *America Comes of Age*; in 1959, Henry F. May pictured a stage of the process in *The End of American Innocence*; most recently, Daniel Bell announced "The End of American Exceptionalism" in 1976. Repeatedly proclaimed, the rites of puberty seem perennially deferred.

"The youth of America is their oldest tradition," wrote Oscar Wilde. "It has been going on now for three hundred years." David Hackett Fischer quotes Wilde to that effect in his forthcoming book, *Growing Old in America*, and calls him to terms for exaggerating by as much as a century. In doing so he has in mind the thesis of his own study, which I think he successfully proves, that there occurred in the late eighteenth century and afterward a "revolution in age relations" in America, a momentous transition from *gerontophilia* to *gerontophobia*. It might be symbolized by the change from powdered wig to toupee. Although they are not the same at all, there are

¹¹ Quoted in Winthrop D. Jordan, "Searching for Adulthood in America," *Daedalus*, 105, #4 (1976): 5.

undoubtedly connections between American attitudes toward youth and age, Fischer's subject, and the myth of the perennially youthful nation, the subject of this paper. Fischer does not explore these connections, but he does remark in passing that "America itself, as late as the mid-twentieth century, still thought of itself as a young nation even though it was more than three centuries old—in fact the oldest independent republic in the modern world."¹² I believe it continues to think of itself the same way in the last quarter of the twentieth century.

Explanations might be sought in numerous theories. The search would surely not overlook what on the surface might seem paradoxical—that unique antiquity and continuity of the American constitutional order, the unbrokenness of the eighteenth-century institutions. We are surrounded by them in Washington, some of them, as Daniel Boorstin has pointed out, housed in the original buildings or reconstructions and expansions of them—notably the White House and the two houses of Congress. Of the same age are the Supreme Court, the two-party system, the ancient names and doctrines, and above all the old Constitution, much amended and reinterpreted though it is.¹³ They all contribute to the illusion of frozen time, of arrested change. "America was the exemplary once-born nation," Daniel Bell observed in the essay referred to above, "the land of sky-blue optimism in which the traditional ills of civilization were, as Emerson once said, merely the measles and whooping cough of growing up."¹⁴ Under the same diagnosis, subsequent ills that are more regularly associated with maturity and aging could be similarly dismissed as infantile disorders of the Emersonian sort.

Another explanation might be sought in the experience and heritage of what I have called "free security" in American history. By that I mean the remarkable amount of military security that this country, unique among nations, enjoyed between the second war with England and the Second World War, largely as a bounty of nature and the accidents of geography and politics. It was a security so free, so exempt from the heavy price other nations had to pay for it, that Americans came to regard free security as a natural right.¹⁵ Just as the infant is normally more secure in his world than the mature man is in his, so, as Reinhold Niebuhr has observed, "a strong America is less completely master of its own destiny [i.e., less secure] than was a comparatively weak America, rocking in the cradle of its continental security and serene in its infant innocence."¹⁶ Nothing could have been more natural than a strong national impulse to recapture or return to that infant security and the innocence that supposedly accompanied it. Innocence was much more illusory than security as an endowment of national youth. The conquest of a

¹² New York, 1977 (read in uncorrected galley proof). See especially ch. 2, "The Revolution in Age Relations, 1780–1820," and ch. 3, "The Cult of Youth in Modern America, 1780–1970."

¹³ *The Genius of American Politics* (New York, 1958).

¹⁴ "The End of American Exceptionalism," *The Public Interest*, #44 (1976): 223.

¹⁵ C. Vann Woodward, "The Age of Reinterpretation," *AHR*, 66 (1960): 1–8.

¹⁶ *The Irony of American History* (New York, 1952), 35, 74. In speaking of "the reality of innocence in our foreign relations," Niebuhr rather uncharacteristically brushed over some exceptions.

continent and the enslavement of a race were not brought off innocently. Americans found it difficult, however, to maintain the national myth that the United States is an innocent nation in a wicked world save by preserving the fantasy of youthful innocence.

Maturity may be legitimately reckoned by experience, but not simply by the amount of it or by the years that pass in acquiring it, by sheer age. The quality and character of experience must also figure in the reckoning, as well as the response evoked by the experience. Our historians fill many volumes with their accounts of the centuries of national experience, the complexities and problems of those years. Much of what they have written sustains the legend of American history as a success story. When it is written largely as an account of the white man's experience, when it neglects the losers for the winners, the story does indeed appear to become a string of triumphs and successes. Americans won their wars and solved their problems and prospered fabulously in the process. And when their victories, their triumphs, and their prosperity—the American Way of Life—are interpreted as the rewards of their virtue, history becomes a morality tale; and the myth of American invincibility, success, opulence, and innocence is seemingly vindicated.

"The American," remarked George Santayana back in 1920, "seems to bear lightly the sorrowful burden of human knowledge. In a word, he is young." Then he added as an afterthought that, of course, "The American has never yet had to face the trials of Job. Great crises, like the Civil War, he has known how to surmount victoriously; and now that he has surmounted a second great crisis victoriously [the First World War], it is possible that he may relapse, as he did in the other case, into an apparently complete absorption in material enterprise and prosperity. But if serious and irremediable tribulations ever overtook him, what would his attitude be?"¹⁷ He continued to watch the American scene from abroad through the remaining years of his long life for an answer to his question about how Americans would respond to "serious and irremediable tribulations." Those of the Job-like seriousness he had in mind never came in his lifetime, of course, but he was not reassured by American hysteria over "a first touch of adversity," as he described the Great Depression of the 1930s. After the Second World War his American friends had scarcely got back home before they began sending him "coloured pictures of happiness, abundance, youth, travel, and laughter [that] have transported me to a sort of dream-world where everything is a merry-go-round . . . a sort of youthful gaiety, as if everybody were dressed in brand new clothes and rushing from one 'delightful' thing to another."¹⁸

It was not that Americans were still miraculously exempt from the great crises that afflicted the rest of the world. Americans experienced them too, but in a different way—remotely. Even the dismal depression years and the Second World War were transformed in retrospect into triumphs of American

¹⁷ *Character and Opinion*, 111, 116.

¹⁸ Santayana to Mrs. C. H. Toy, July 27, 1932 and to Mrs. David Little, September 23, 1949 in Daniel Cory, ed. *The Letters of George Santayana* (New York, 1955), 275, 384.

power and will. Before their meaning had ever sunk home, God or History or Luck had, as always, come along and bailed them out with some ego-transporting triumph like the world-wide victories of 1945, the temporary monopoly of the ultimate weapon, and the Pax Americana—together with fabulous increments of national wealth and power and prestige and enhanced illusions of national virtue to boot. So that following hard on a withering winter for an aging faith came yet another spring, and the old creed seemed to be re-greening again and to flower more luxuriantly than ever for all its centuries of aging.

In remarking that Americans had displayed the ability “to surmount victoriously” great crises like the Civil War, Santayana failed to mention a large number of Americans who did not surmount that particular crisis victoriously. Their experience was quite different. The South as loser sustained historic encounters with defeat, failure, poverty, and guilt that embarrassed its later efforts to embrace the national myth of invincibility, success, opulence, and innocence. It was an experience more characteristic of the world’s “sorrowful burden” from which Santayana thought Americans immune. This heritage found lodgment in the folk mind of the South and eventually gained expression in the work of a few of its brilliant writers. But that was not readily exportable, and the Northern market for it was limited. Even the French market was greater. What came through to Yankeedom instead was the spurious “New South” nationalism of the new ruling class, the plantocracy’s bourgeois successors. So the South has failed its fellow countryman in a service of potential importance and even failed its own legitimate heritage in embracing the leadership and the spurious myths it has.

There were other losers, other Americans with a collective experience at fundamental variance with national myths. They included some of the oldest elements of the population, the enslaved and persecuted black people pre-eminently and other humiliated minorities of color, along with the rest of the impoverished. They also had something of importance of a cautionary nature to tell their fellow Americans about national pretensions of immunity from the forces of history, the faith that Americans were the darlings of divine destiny, and the delusion that they could control history for their own ends. But our own humiliated minorities have never been able to teach us what they learned at our own hands. The less wonder that, since the greatest teacher of humility in the history of religion never really learned how to teach humility very successfully to those who had never known humiliation themselves.

To put the mythic American experience in broader than domestic perspective it would be necessary to extend comparisons much wider than is possible here. We can only suggest the possibilities by thinking of the course of world history during the two centuries the conclusion of which we have just celebrated. When one thinks of the horror and anguish, the humiliation and tragedy that those two hundred years have inflicted upon the great peoples of the world—the Chinese, the Russians, the Japanese, the people of Indochina, the Hebrews, the Hindus, and the Muslims, the indigenous peoples of Africa

and the Americas—even the nations of Western Europe, all of which have fallen under the heel of foreign military occupation at some time during that period—only then does one begin to appreciate the historical context of what has been called “American innocence.” In all that long period, though America learned to contribute to those foreign horrors, never once, save in what was really an extension of our domestic quarrel with England, have foreign invaders set foot on American soil. No military occupation (save that of the South by Americans), no devastated cities (with the same exception), no mass deportation, mass rapes, mass executions, no class liquidations, no crematory ovens.

And surely no American would have it different, even for the loftiest educational reasons or the most sophisticated Machiavellian purposes. Besides, there is no assurance that the school of adversity teaches the right lessons. Nor does an adverse childhood assure any quicker or riper maturity than a happy one. Quite the contrary too often. The brief history of many of our junior republics testifies amply to the same unfortunate results among nations. It is doubtful that the defeated South was ever able to make its heritage of adversity contribute substantially to its maturity and wisdom save among a few individuals. That is not the point. The point is the skewed or eccentric perspective on history that our unique good fortune during most of our two centuries of national existence has fostered in America. That plus a deeply ingrained set of expectations that are scarcely the ideal preparation for the adversities normally encountered in, though not necessarily caused by, the course of aging. They would include some of what our Spanish philosopher called the trials of Job. As I have pointed out already, Americans have largely been spared those so far. But history may be presumed to have them still in store for us, and come they will in time.

AS IF IN ONE OF ITS MOODS OF PLAYFUL IRONY, history recently provided Americans with some relatively mild but instructive foretaste of the possibilities. The ironic turn of events came in the timing of the samples—just on the eve of our national celebration of two hundred years of unique good fortune that was the reward of virtue and righteousness. For one thing, the United States of America actually suffered defeat in a war. The implications are worth spelling out a bit, for it was the first war popularly perceived and partially admitted to have been lost. True, it was not a major war and it was fought on the opposite side of the globe without the slightest physical danger to Americans at home. Still it was a defeat, and as such a shattering blow to the national myth of invincibility. Few face-saving opportunities were available to temper the blow. It came at the peak of American military power and pride and was sustained at the hands of a weak, undeveloped, and relatively unheard-of country in Southeast Asia crippled by civil war and under Communist rule. Eventually faced with a mutinous and demoralized army at the front and rebellion at home, the defeated were compelled to do many of the

things traditionally required of the vanquished—eat their words, swallow their pride, and do that which they had solemnly sworn they would never do. It was, on the whole a most un-American experience. “Peace with honor,” it was called. Perhaps the bitterest pill to swallow was the exposure of the emptiness of moral pretensions used to justify the war and the shame with which American foreign policy was covered in the eyes of the world.

The ironic turn of events was not exhausted by introducing the bicentennial anniversary with disgrace abroad, for accompanying that misfortune and coinciding with its climactic phases occurred the exposure of by far the worst domestic disgraces in the history of American government. Starting in the White House at the very top, a spirit of lawlessness—condoning conspiratorial deception, obstruction of justice, bribery, burglary, thuggery, perjury, a great variety of high crimes and misdemeanors—infested a whole administration. Though the crimes were often sordid and self-serving, their perpetrators defended themselves in a self-righteous spirit. Implicated and often convicted of crimes were some of the chief governmental officials, including two who had served as the highest law enforcement officers of the government, both of them as United States attorney general. Acting personally and through his subordinates and agents, the president misused for his political ends the Internal Revenue Service, the Federal Bureau of Investigation, the Secret Service, and the Central Intelligence Agency. Few agencies of the executive branch of the government escaped the corrupting influence. The House Committee on the Judiciary, after hearings of unprecedented length, found the president had “acted in a manner contrary to his trust as President and subversive of constitutional government, to the great prejudice of the cause of law and justice and to the manifest injury of the people of the United States,” and declared that “such conduct warrants impeachment and trial, and removal from office.”¹⁹ Further disclosures were frustrated by the resignation and pardon of the president.

Ironic preliminaries of the bicentennial celebration were further compounded, however, by disclosures in other fields. Hard upon the vote for impeachment and the resignation and pardon of the president came charges of gross and frequent abuses of limited authority by intelligence agencies that made our own citizens targets of espionage. It was eventually confirmed that for twenty years beginning in 1953, the CIA had been illegally opening first-class mail, had in fact handled more than four million items in 1973, presumably its last year of operation in this field. In addition, the CIA maintained personal files on 10,000 citizens, a computer system containing 300,000 names of American organizations and citizens with no apparent connection with espionage, and a watch list of some 1,000 organizations. It also engaged in unconstitutional wiretapping, bugging, and illegal examination of tax returns, and administered LSD to unsuspecting Americans.²⁰ As twin Big Brother of

¹⁹ *The Impeachment of Richard M. Nixon, President of the United States, Report of the Committee on the Judiciary*, House Document No. 93-339, 93rd Congress, 2d Sess. (Washington, 1974), 3.

²⁰ United States Commission on CIA Activities within the United States, *Report to the President* (Washington, 1975), 19-36.

the CIA, the FBI was revealed to have been busy with comparable activities and to have been particularly zealous in hounding and persecuting leaders of protest and peace movements. Contemporaneous investigation at home and by foreign governments revealed that some of the largest American business corporations, including some with heavy government subsidies, have made a practice of bribing foreign officials with enormous sums. Disclosure of these bribes could topple friendly governments and threatened to topple one monarchy.

None of these American misfortunes such as Vietnam and Watergate should be thought of as inevitable or necessary symptoms, afflictions, or consequences of aging. Malaise, to be sure, illnesses of several sorts. But to my mind they do not evoke, as they have for some, images of Diocletian and late Roman calamities. Worse things have happened in recent times to regimes much younger than America's—for example those of India, China, or Russia—without setting off Spenglerian reflections. The only special correlation with age is that of being around long enough, which naturally incurs greater risk of getting in the way of such things happening. It is not that they are historical anomalies, but that they are anomalous in the history of America. And there is quite enough odor of brimstone about them—about what is known, let alone what is still undisclosed—to prompt renewed speculations regarding George Santayana's question of how Americans, in their frozen posture of innocence and youth, would respond if ever overtaken by "serious and irremediable tribulation." Odds are that old Job would have shrugged them off as trivial, and they have not yet been proven to be irremediable. But as I say, more than a whiff of sulphur still lingers about them, enough at least to suggest the test of Job's trials.

In their tortured response to the Vietnam War, the longest and most divisive in their history, Americans were divided ideologically between adherents of two main tenets of the American myth—that of invincibility and that of innocence. Many were caught somewhere in the middle, torn between the two, but virtually all—and especially those of the right and the left—adhered to the myth in some form. Behind the tenet of invincibility were aligned those of the right to whom the only conceivable end to an American war was victory. A defeat or a "no-win war" was to them so abhorrent that they rejected all thought of compromise, even negotiation, and were prepared to incur any amount of guilt, even resort to atomic weapons, to gain their ends. On the opposite side were those who, in addition to their genuine abomination of an unjust war, were so dedicated to the myth of national innocence that their abhorrence of guilt exceeded any concern for defeat. To restore lost innocence or stop the increment of intolerable guilt, some would renounce all future resort to force and military means in any circumstances. The myth of innocence had special appeal for the new cult of youth, including the counterculture then engaged in yet another greening of America. The clash between the new cult of youth and innocence and the older culture of invincibility and success further intensified ideological warfare at home, but the struggle was for priority of myths, not for their renunciation.

The revelations of Watergate were an especially rude challenge to the myth of American innocence. So repugnant and gross were they that there was a strong disposition to disbelieve the plain and undeniable evidence or to connive tacitly in a conspiratorial cover-up. On April 25, 1973 there occurred an unforgettable telephone conversation between President Nixon and H. R. Haldeman. "Despite all the polls and all the rest," said the president, "I think there's still a hell of a lot of people out there, and from what I've seen they're—you know, they, they want to believe, that's the point, isn't it?"²¹ He was right. They desperately wanted to believe, and they were prepared to go to almost any lengths to shore up the faith—innocence by "stonewalling," if it came to that. Had it not been for the unprecedented quantity and explicit character of the evidence—millions of words recorded electronically in the very tone, accent, and characteristic vocabulary of the accused—there can be little doubt who would have presided as head of state over the bicentennial year. Once the stonewalling ceased and the evidence was confronted, the old institutions of Senate, House, and federal judiciary, moved with gratifying firmness to perform their constitutional duties. Even so, even before all the trials were completed, the verdicts handed down, and the culprits sentenced or pardoned in advance, a strong national disposition was manifest to put the unfortunate matter out of mind and get on with business. In some circles it was considered poor taste to bring Watergate up at all, and in the ensuing presidential campaign the subject was virtually banned.

Although none of these ironic pranks that history played as prelude and accompaniment to the American Bicentennial can really be classified with old Job's "irremediable tribulations," neither can they be dismissed as Emerson's "measles and whooping cough of growing up." Yet that would seem to be the predominant diagnosis—a problem for pediatrics, not one for geriatrics. The refusal to grow up can have serious consequences, and Americans still show symptoms of refusal. They would seem to stand poised on the brink of the third century of the national era and the third millennium of the Christian era, determinedly youthful, stonewall innocent. Or is it that in their private reading of the Book of Job Americans skipped the meat of the thing, missed the meaning of submission, and jumped to the epilogue? For there, you will recall, we are told that the Lord not only restored Job's fortunes but doubled his possessions. For good measure, the Almighty endowed him with fourteen thousand head of cattle, six thousand camels, a thousand yoke of oxen, a thousand she-asses, seven sons, three daughters of unsurpassed beauty, and another hundred and forty years of life to enjoy it all.

²¹ Unpublished court transcript quoted in Theodore H. White, *Breach of Faith: The Fall of Richard Nixon* (New York, 1974), 327.

Comments:

TO HIS PROFESSIONAL COLLEAGUES Vann Woodward's admirably lucid paper may well be more striking for the way it approaches its subject than for its argument. I do not mean to deny either the significance or the credibility of what he says about the celebratory rhetoric and behavior of some Americans—even if they are professionally articulate Americans—during the bicentennial year. It is useful to know that many of us still like to portray ourselves as a people of unique innocence and invincibility—a people inhabiting the land of youth and new beginnings where things happen first. It is useful, but not surprising, to learn that many Americans remain enthralled by that familiar self-image, or that it seems to tighten its grip on the imagination as it comes to bear less and less resemblance to actuality. Few historians are likely to quarrel with the main point about this flattering self-image, namely, that it amounts to a kind of lie—a beautiful lie to be sure, but nonetheless a distortion of the truth or (as some would say) a “myth” or, as Woodward puts it, a “skewed or eccentric perspective on history” fostered by the unique historical good fortune of the American people. All of this is perfectly credible, for the fact is that we have been hearing this characterization of the collective mentality of Americans for a long while now.

What is striking, however, is to hear this argument from Woodward, for in making it he adopts a method—a way of doing history—with which he has not hitherto been associated. Indeed it is an argument and a method generally associated with a small, some would say idiosyncratic, group of scholar-critics with a strong literary bent—a group we do not ordinarily think of as belonging to the mainstream of historical research conducted by members of university departments of history. On the contrary, most of the professors whose insights Woodward so generously credits and seemingly corroborates belong to departments of English or to American studies programs; if they have been associated with the guild of professional historians, it is likely to have been through the interdisciplinary American studies movement. Their method, to be more specific, is in large measure traceable to the work of Henry Nash Smith, especially *Virgin Land* (1950), and, beyond that seminal book, to a long line of commentators upon American life, including D. H. Lawrence, who developed a way of examining our national experience through the lens provided by the work of our greatest poets, essayists, and writers of fiction—that is, by classic American literature. Although practitioners of this method do not necessarily confine their attention to literary evidence, they have assumed that a national literature is an expression of national experience, and

their organizing categories have tended to be literary, hence the delicately derisive label: “the image-symbol-myth” school of American studies.

As a sometime member of that school, I must confess that I read Woodward’s paper with mixed emotions. On the one hand we have been done a great honor. We hardly can feel anything but pleasure and gratitude at the prospect of claiming so distinguished a convert. And yet, at the risk of being presumptuous, I am compelled to warn him that he is dangerously close to the very methodological trap from which we are trying to escape. If the first half of the essay can be taken as testimony to the usefulness of our method, the last half exhibits its deficiencies. The problem, as Woodward clearly recognizes, is not to become a captive of the very figure—in this case the “aging” of the Republic—which is his subject. But it is not enough, we have discovered, for the historian to remind himself and his readers that his subject is, at bottom, a mere analogy or metaphor—a figure of language not to be taken literally. To escape entrapment it is ultimately necessary to have some other, firmer, ground on which to base one’s analysis—a realm of being that is more solid, more “real,” in the sense of possessing more demonstrable efficacy in the lives of men and women than such exclusively mental constructs as the dubious notion of the youth and age of nation states.

Perhaps we can clarify the point by reminding ourselves that in essence the problem here is epistemological; and it has beset the “image-symbol-myth” school from its inception. Henry Nash Smith defined it in the notorious disclaimer he inserted in the preface to the first edition of *Virgin Land*. Referring to the images, symbols, and myths (or “collective representations”) about which he was proposing to write, he said, “I do not mean to raise the question whether such products of the imagination accurately reflect empirical fact.” But that is precisely the question he repeatedly raises. As some readers were quick to notice, again and again Smith compares these products of the imagination with, if not “empirical fact,” well then, something more like social behavior.¹ Indeed, the illuminating power of his method is indicated by the bright light it casts on the discrepancy between the kind of society Americans believed (or professed to believe) they were creating (a new garden of the world) and the kind they in fact were creating (the world’s most powerful urban industrial capitalist society).

Although Smith’s initial disclaimer is misleading, it also is extremely helpful. Insofar as he denies any attempt to compare collective representations (images, symbols, myths) with another, presumably more “real,” dimension

¹ In the new preface to the twentieth-anniversary printing of *Virgin Land* (Cambridge, Mass., 1970), Smith acknowledges that his earlier disclaimer “encourages an unduly rigid distinction between symbols and myths on the one hand, and on the other a supposed extramental historical reality discoverable by means of conventional scholarly procedures.” But Smith fails to settle upon a more satisfactory definition of that other dimension of experience which he initially had referred to as “empirical fact” and now calls, with characteristic skepticism, “a supposed extramental historical reality.” I am arguing that the other entity is in fact an unacknowledged *conception* of the character and functioning of American society. The central place of this conception becomes especially apparent in Smith’s clear, unequivocal treatment of the Turner thesis in the closing pages of *Virgin Land*, but in his accounts of his method Smith does not match his practice in this regard; he does not, that is, acknowledge the centrality of his tacit theory of society.

of experience, he surely is misleading. But in hypothetically identifying that other entity as “empirical fact” he points directly to the essential problem. My impression is that here Smith’s underlying method is weakened by the influence of the reigning ideology of the American academy: a form of empiricism. He assumes that, if we are to test the validity of mental constructs, it will be by comparison with a reality immediately visible and observable in the historical data—that is, empirical fact. But on close examination of *Virgin Land* that other entity proves not be anything like an empiricist’s notion of the given, the concrete, pure data, or “empirical fact,” but rather (although Smith never acknowledges it) an embryonic *concept* of the character, structure, and functioning of American society. Smith’s book could not have been written without a sophisticated conception of the transition between a small-property agricultural economy and highly industrialized corporate capitalism. And this conception, needless to say, he did not derive either from “empirical fact” or “extramental historical reality.”²

My point is that much of the work done by practitioners of this American studies method has been vitiated by our failure to acknowledge the indispensibility of a clearly articulated concept of society. In some cases the concept has simply been missing, in others it has been present to a degree but largely unacknowledged. The result is that the “school” has developed considerable skill in relating individual products of mind (works of art and scholarship—expressive forms of all kinds) to large collective mental formations—myth, ethos, ideology, world view, and the like. But, until we can apprehend these formations as they are related to a concept of social structure and its operation, they will continue to seem unmoored—a ghostly, free-floating cloud of abstractions only distantly related, like the casting of a shadow, to the actual struggles of everyday life.

Returning now to Vann Woodward’s paper, I note that his analysis of recent American history suffers from a similar tendency to reify ideas, including figurative conceptions like the aging of nations. Although he asserts his independence of the aging metaphor, he actually falls back upon it to describe the assorted horrors of the Vietnam era—not only the war but the Nixon presidency, Watergate, the CIA scandals—as an ironic foretaste of the “trials of Job” which, he prophetically contends, history still holds in store for the American people. The adversities that Americans have recently experi-

² The propensity of Smith and other Americanists to bypass social theory and posit, as the entity against which to test the validity of mental constructs, a realm of “empirical fact” or “extramental reality” is a cultural phenomenon of some importance. It might be argued that this propensity reflects the influence of the very world view under study in *Virgin Land*. Thus, in chapter 16 (“The Garden and the Desert”) of that book, Smith provides a remarkable if rare example of a collective belief (in America as a garden) tested against “empirical fact” (the annual rainfall on the great plains). I say “rare” because cultural historians have few such opportunities to hold ideas up against the realm of verifiable, quantifiable, biophysical fact. In this unusual case Smith’s method seems to comport with the belief, fostered by the myth, that external nature (the biophysical environment) is the locus of a more reliable “reality” than either collective mentalities or modes of social organization. To the degree that chapter 16 exhibits the ideal model of Smith’s method, one might conclude that he has (and, in varying degrees, the scholars whom he has influenced have) allowed this characteristically American belief in “nature” as a repository of meaning, value, and truth to skew the conception of the historian’s method.

enced, he says, are of the kind “normally encountered in, though not necessarily caused by, the course of aging.” Just here, at the point where we would expect an historian to provide his own account of the current transformation of American society and government, Woodward relies on the aging metaphor as an explanatory concept.

A resort to image, symbol, and myth as the chief bases of historical explanation is the prime deficiency of this American studies method, and I fear that Woodward exhibits a similar tendency in his analysis of national behavior during the Vietnam War. He accounts for the responses of the American people to that disastrous war almost exclusively by their presumed adherence to one or the other of the two main tenets of the national myth: invincibility or innocence. Right-wing supporters of the war, he argues, were so committed to the doctrine of American invincibility that they demanded victory at any cost, whereas left-wing opponents of the war were so dedicated to the myth of national innocence that their “abhorrence of guilt exceeded any concern for defeat.” No doubt there is some truth in this explanation. But it is troubling to see how little room the even-handed historian leaves for the possibility that some Americans, in their responses to our Ten Years’ War in Southeast Asia, were activated by ideas, perceptions, or motives other than those which derive from the myth. If the myth is responsible in this case, it presumably controlled our responses to other wars. How, then, shall we account for the unprecedented militancy of the opposition to this particularly unjust war? It is difficult to believe that the American people are quite such mindless prisoners of illusion as Vann Woodward implies. There are good reasons, I believe, for crediting them with at least vestigial degrees of political intuition, common sense, and understanding.

What is involved here, I think, is the sufficiency of the analyst’s own conception of political behavior and societal reality. What sort of war was the American war in Southeast Asia? Even if we were to grant (as I cannot) that the struggle about the war within the United States is largely to be understood as “a struggle for control of very old myths,” it would still be incumbent upon the historian to step out of the framework of the mythic consciousness at some point if he means to establish its illusory character. But Woodward scarcely does. So far as he characterizes the Vietnam War directly, in his own voice, he adheres largely to the idiom of “victory” and “defeat”—language very close indeed to that fostered by the myth of national invincibility. At no point does he suggest that the Vietnam War was an episode within a much larger conflict—a conflict between the United States and a revolution (or series of revolutions) of well-nigh global proportions.

To expose the masking effect of the myth, the historian must finally identify the actualities it masks. In this case it masks a ruling concept of American foreign policy—namely, the urgency of the global power struggle within which the invasion of Southeast Asia by the armed forces of the United States was merely one episode. Since the beginning of the cold war we have been engaged in an unflagging effort—sometimes clandestine (as in Brazil, Chile, or

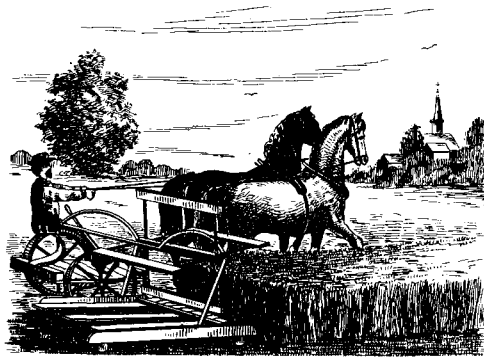
Greece), sometimes paramilitary (as in Cuba or Guatemala), and sometimes military (as in Vietnam, Laos, and Cambodia)—but in any event a remarkably consistent effort to turn back the revolutionary wave which has swept across large parts of Asia and Africa and has threatened a number of countries in South America and Europe. In this counterrevolutionary campaign our government consistently allies itself with authoritarian regimes of the right, justifying that policy on the ground that the various revolutionary movements are inhospitable to Western democracy (and capitalism). When the Vietnam War is seen from this larger perspective, and when it is related to recent systemic changes within the United States, then, in my judgment, we shall be in a position to appreciate fully the distorting power of the puerile myth of national invincibility and innocence.

I dwell upon Professor Woodward's treatment of the Vietnam War not to pick a political quarrel, but rather because it so vividly exemplifies an inherent weakness in the "image-symbol-myth" method of doing history. The historian sets forth with a clear notion that he is writing about a mental construct, perhaps a mere analogy like that between the innocence or aging of persons and of nations. But, unless he is willing to commit himself to a concept of social reality distinct from the putative mythologizing of the collective consciousness, he is in serious danger of falling victim to the myth whose illusory character he would expose to view. Until we develop more refined and reliable ways of establishing the interaction between cultural formations and the structure of wealth, status, and power (in shorthand, the social structure), this particular American studies method is likely to remain in its present state of arrested development.

But that is all the more reason for welcoming to our ranks so distinguished a practitioner of hard-headed, sociopolitical history as C. Vann Woodward. We need the help of such historians, scholars who appreciate the importance of the evidence on both sides of the great epistemological divide. To understand a society and culture as a whole, we must recognize how profoundly it is affected by the way its people perceive the world (including, of course, themselves); and yet, at the same time, we must recognize how untrustworthy those perceptions may be. This is only to say that we must somehow contrive to know the society both *through* and, as it were, *around* the collective consciousness; and we must try, above all, to understand the "fit" between the two kinds of knowledge. But we will be able to do so only if we posit a conception of society that takes far more account of its structure than is revealed by any conceivable accumulation of "empirical fact" or by any image, symbol, or myth, however compelling.

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AS ALWAYS, C. VANN WOODWARD'S REFLECTIONS are both wise and provocative. He calls attention to the paradox of our celebrating a two-hundredth birthday as if it were a bar mitzvah or a first communion. He notes how we have ignored or suppressed recollections of suffering, and he leaves us with the question of whether the anguish of the 1960s and 1970s, occasioned by humiliation in war and the deposing of a president, will or will not have enduring effects on our national sense of self. Though in no way quarreling with the points that Woodward makes, I feel obliged to ask whether one can properly take the bicentennial observances as an index to how Americans perceive their collective experience. Does this celebration, like the Centennial of 1876 or the Columbian Exposition of 1892, provide a momentarily frozen profile of the American physique and character? Does it really lend itself to the type of analysis at which Leo Marx displays such skill? I doubt it.

For most people in the world, national history has or can have some correspondence with family history. The village of Corton in Burgundy produces a great white wine called Corton-Charlemagne, taking note of the fact that the vineyards once belonged to the Emperor Charlemagne. For many people in the village, events in the history of Burgundy and France in the intervening twelve hundred years could well be remembered as ones that involved or affected their ancestors. Nor is this the case only for citizens of what Vann Woodward terms the first generation of modern Western nations. In Mexico, the great anthropological museum in the national capital has a collection of Toltec, Aztec, Mayan, and other Indian artifacts which are part of the actual heritage of a majority of Mexicans. Buildings and monuments in almost every city and town provide reminders of the sixteenth-century Spanish conquest which is also in that heritage. Of one of the newest African nations, still in the throes of national birth, Basil Davidson writes,

. . . an ethnologist began asking old men who lived in the rolling grasslands of the Mount Darwin district, north of modern Salisbury, whether they knew anything of the distant past. They hesitated and then they began telling him the history of their people and its kings. They went back to Mutota, the first of their strong rulers, who was "still the heart of the nation" and whose burial ground was the hill of Chitakochangoya. They admitted that "we no longer talk about these matters very much, now that the Europeans have taken the place of Mutota's sons." But they remarked that there were elders still alive . . . "who say that if you listen carefully you can hear the roll of Kagurukute, the great drum of Mutota, at the time of the new moon, as you

stand looking down the river Dande, beside the lofty grave." Now Mutota had died in about 1450. The old men were recalling five centuries of statehood.¹

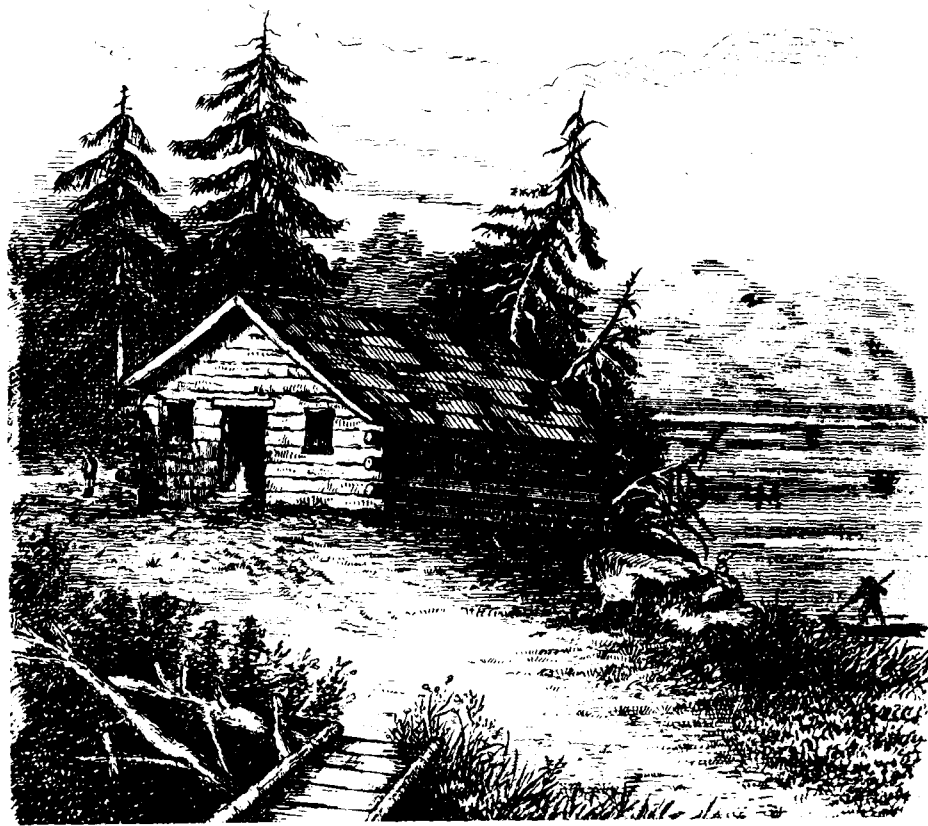
Here in the United States old men reciting family lore would be most unlikely to talk of the Boston tea party, Lexington and Concord, or the exploits of General Washington. In Massachusetts, where I live, plenty of quaint costumes and antique weapons, implements, and vehicles have been on display, but the men and women involved in the pageantry were not for the most part ones whose household histories comprehended the events being recreated. The mayor who welcomes tourists to Boston comes from a family in which the significant history includes the Tithe War and the Great Famine, and the remembered heroes are O'Connell and O'Brien. The governor of the state is from a family that was involved in the struggle for the liberation of Greece. A friend of mine who, in ornate costume, galloped through Middlesex as Paul Revere is in real life a building contractor whose ancestors fought alongside Garibaldi. In Massachusetts, as in certain other places along the Atlantic Coast, there are physical survivals that may establish some sense of connection with the colonists' revolt against England. In most parts of the United States, however, nothing at all lends reality to that set of events.

One of the most elaborate state programs for the Bicentennial was that in Texas. I find that fact slightly comical, for I was born and brought up in Texas, and I recall almost nothing from my own childhood and school years that relates to the American Revolution. It is possible, though not certain, that the ancestors of one of my grandparents included a Virginian who took part in the Revolution. In that branch of the family, however, the centerpiece of the recalled past was the Civil War and the subsequent migration westward from Mississippi. Another grandparent was of an English family that came to the Texas Republic in the 1830s. A third was descended from German Forty-Eighters. The fourth was a thirteenth generation heir of a Spanish soldier who received a grant of land in the valley of the Rio Grande from King Philip II twenty years before the English planted themselves at Jamestown.

Neither among my kin nor in school was the American Revolution given significance for me. The Texan Revolution was something else. We re-enacted it in class. There were tours to the Alamo and the battleground at San Jacinto. And I thought of the war between the United States and Mexico as having determined my language and nationality. Not until I took up craft as a historian did the American Revolution become an event of much interest to me. Though conceding that Texas's reputation for state-chauvinism may not be wholly undeserved, I venture the proposition that, for most Americans, the American Revolution touches no personal chord. It has little or nothing of the power which the Civil War has, as Woodward notes, for nearly all native Southerners.

This being the case, was it not inescapable that the Bicentennial should have been observed as it was—in drama and pageantry recreating the eigh-

¹ *The African Genius: An Introduction to African Social and Cultural History* (Boston, 1969), 45-46.



teenth century and seeming thus to celebrate youth? Can any significance attach to the fact? To raise this question is not, of course, to dispute Woodward's central point. There is plenty of other evidence to support the contention that we tend to think of the United States as a young country, despite its age, and that many—perhaps most—of us persist in exaggerating its power or its purity or both. It may be, however, that these traits are also in part functions of our lack of a long common history. Immigrants were mostly young. America was thought of as too taxing a place for the old. Immigrants and their children spoke of the lands they had left as “the old country,” which in itself made America “the young country.” And immigrants who stayed, made homes, and reared families easily absorbed the myths of omnipotence and innocence; for, as Carl Becker wrote in his essay on Kansas, “To venture into the wilderness, one must see it, not as it is, but as it will be. The frontier, being the possession of those only who see its future, is the promised land which cannot be entered save by those who have faith. America, having been such a Promised Land, is therefore inhabited by men of faith.”² And the point holds as much for frontiers such as the South End in Boston or Chinatown in San Francisco as for the prairies of Kansas.

By and large, Americans in the bicentennial year did not reflect on two

² *Everyman His Own Historian: Essays on Politics and History* (New York, 1935), 12.

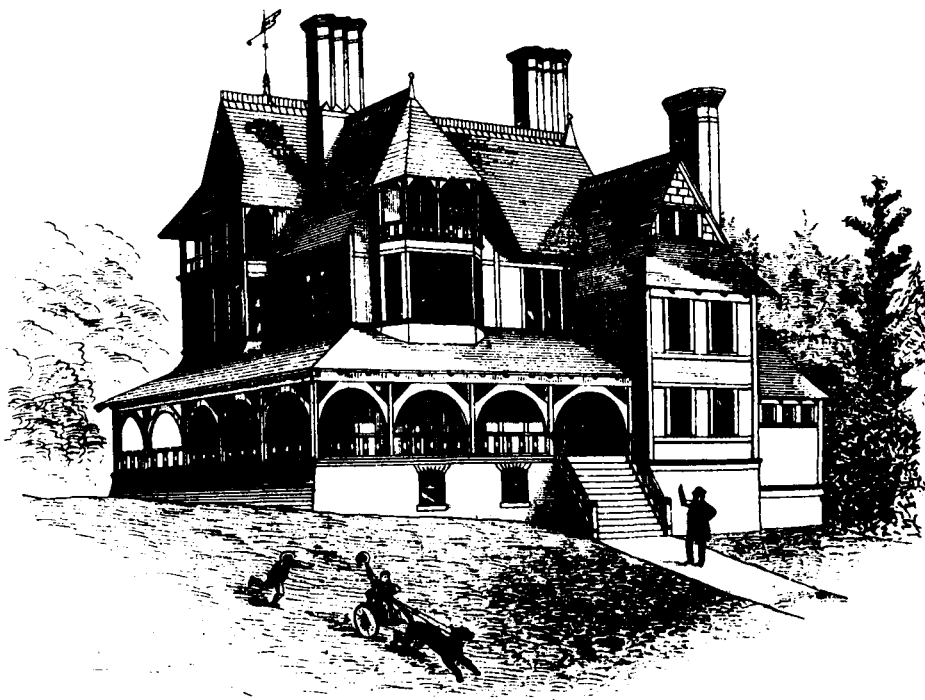
centuries of age and experience because few of them had shared those two centuries. They had suffered—but not together. The famines and riots, the visions of foreign soldiery tramping the streets, belonged to other places, other times. They were experiences put behind in the new beginning. Except for Southerners, the first World War is probably the most remote event in our national history that has some meaning in the personal or family histories of a majority of Americans. Our common national history may, indeed, begin with the Great Depression. If that is so, then it is not surprising that we should continue to think of our nation as not yet mature, not yet fully formed, not yet wholly ripe in wisdom.

The recent events to which Professor Woodward refers—Vietnam and Watergate—may, however, have the effect of hastening a sense of ripening. We shall have to wait and see. I am reminded of a story I heard not long ago from a retired general. He told of an old man in his home town who was thought to be wise and was hence consulted by everyone. When asked by someone else how one acquired a reputation for wisdom, the old man replied: “Well sir, wisdom is a product of good judgment. Good judgment is a product of experience. And experience is a product of poor judgment.”

In the last ten years, we as a nation have accumulated a lot of experience.

ERNEST R. MAY

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Reviews of Books

GENERAL

WILLIAM H. MCNEILL. *Plagues and Peoples*. Garden City, N.Y.: Anchor Press. 1976. Pp. viii, 369. \$10.00.

GEOFFREY MARKS and WILLIAM K. BEATTY. *Epidemics*. New York: Charles Scribner's Sons. 1976. Pp. xii, 323. \$10.00.

The direction of historical research tends often to reflect current problems and anxieties. The conquest of the major epidemic diseases, the increase in population, and a growing Malthusian pressure inevitably force themselves on the attention of scholars. The result has been a growing interest in historical demography and increasing attention to factors, physical and behavioral, which have the effect of promoting or restricting human procreation.

These two books are part of this reaction to a contemporary crisis. Both cover the history of epidemic disease from earliest historical times to the recent past. Both make a case for disease as a factor of major importance in historical change, and both chronicle the major recorded instances of epidemic disease since ancient times. Here, however, their similarities end. In their perceptiveness and in their depth of historical understanding the contrast between them could hardly be greater.

Epidemics is a piece of medical journalism. Its authors catalogue and describe epidemic diseases from biblical times to the recent outbreak of Lassa fever. The clinical discussion of recorded medical evidence is interesting and useful, and the diagnoses offered are of value to the historian. It seems likely, however, that the nature of some historically important diseases, Thucydides' Athenian plague and the English "sweat" of the sixteenth century, for example, is likely to remain obscure. On the other hand, the use made of historical sources is quite uncritical. Ancient and biblical estimates of mortality are accepted and quoted, however unrealistic they may be. Nor does *Epidemics* address itself seriously to the problem of the

historical significance of disease in determining the size of a population or in shaping social attitudes. In the few known instances of disease influencing a military campaign or the course of events, its effects are expressed so categorically that their discussion has little value for the historian.

Plagues and People is a different kind of book. It marshals a large body of clinical evidence, though without the voluminous quotations which are a feature of *Epidemics*, to support a complex argument which gives unity to the book. In brief, William H. McNeill's theme is that most infectious diseases originated in one of several disease pools, of which intertropical Africa and southern and southeastern Asia were among the most important. Within these regions specific diseases had been endemic since the earliest phases of human history, but long exposure had given the inhabitants of these regions a degree of immunity to them, so that they have not been either epidemic or particularly lethal. Immunity is acquired by exposure to a disease and the consequent development of antibodies which fight it. The author perhaps makes too little allowance for mutations of viruses and bacteria and also for the fact that natural selection alone may lead to the emergence of a group resistant to a specific disease. The converse of this argument is the extraordinary virulence of any disease when it attacks a "virgin" population. The bubonic plague of 1347-51 is a classic case. The devastating nature of the outbreak of syphilis which followed the siege of Naples in 1495 is prima facie evidence that it was a new disease, thus supporting the view that it was of New World origin.

The consequence of the creation of "one world" with its close net of transportation and communication has been to fuse the disease pools which had previously been able to exist separately and apart from one another. McNeill sees the period of the Roman Empire as a time when the barriers to the diffusion of diseases from their original hearths were broken down by trade and human movement. To this he ascribes the numerous epidemics

of the period, including Justinian's plague, and in it he finds one factor in the decline of the late classical world. Another such period was that of the great discoveries, when Europe was reinfected with the bubonic plague, and when other diseases, including smallpox, were diffused through Europe. The late medieval plague was carried from Inner Asia in the baggage of merchants and the holds of their ships until it died away in northern Europe for lack of victims in which to live.

A disease can survive only in a society which is large enough for it always to have a susceptible population. A small community is either wiped out by it or it develops a total immunity so that the disease disappears. This immunity is then lost in the course of time so that the whole population is again at risk.

A subordinate theme, interwoven with the main argument, is the psychological and social effects of epidemic disease. The spread of Christianity, it is claimed, was facilitated by the high "die-off" and the greater religiosity which it engendered. Conversely the rationalism of the eighteenth century could only have developed at a time when epidemic disease was being overcome; the last severe outbreak of the plague, it is worth noting, was in Provence in 1720. The features of Buddhism and of the Indian caste system are also related to the experience of epidemic disease at the relevant times and places. This is highly speculative, as the author admits, but the evidence for mental stress induced by epidemic disease is too great to be dismissed.

Plagues and Peoples is challenging more in its insistence that epidemic disease played a significant role in economic and social history than in its attempts to link it with specific trends in intellectual history. The chief consequence of disease was surely that it killed people, that populations were suddenly reduced, and that age-specific mortality could by a kind of echo-effect be felt generations later. Neither book concerns itself with this highly important subject, beyond citing again the oft-quoted estimates of mortality from certain diseases. Sources are few and localized, but it is possible to derive from baptismal and burial registers in Western Europe some evidence not only of the virulence of specific epidemics, but also of the age-specific features of the resulting mortality.

Lastly both books raise the question of "new" diseases, and are unanimous in their conclusion that epidemics not hitherto experienced may be fed into the human biotic system both by mutations of known bacilli and by the transmission of diseases from animals. The disturbing conclusion is that the confluence of disease pools, which was responsible for the most severe epidemics of the

past, is still not complete, and the worst may be yet to come.

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MICHEL FOUCAULT. *Surveiller et punir: Naissance de la prison*. (Bibliothèque des histoires.) Paris: Éditions Gallimard. 1976. Pp. 318.

This book deals less with the history of prisons than with ideas of social discipline and especially with notions of "supervision and punishment" which prevailed in Europe from the sixteenth to the late nineteenth century. In it Michel Foucault continues his attack on the view of the Enlightenment as an unambiguously progressive era in the history of the human sciences. His main thesis is that "the birth of the prison" (the substitution of incarceration for torture as punishment) was as much a reflection of a new, and more oppressive, conception of man and of social discipline as it was of a more humane notion of the person. In the prison, the bourgeoisie sought to create a living model of what life on the outside *should be*. As in the schools and military organizations of the nineteenth century, so too in the prisons "discipline" was established by which to produce a type of citizen that was docile, hardworking, regular in his habits, productive, pious, and above all self-regulating. Thus, although the new judicial system of the nineteenth century replaced the older punitive notion of *lex talionis* with the more humane ideal of reform and rehabilitation, behind the system and serving as its enabling presupposition was a peculiarly repressive conception of both human nature and the ideal social structure. The gain in humanness was offset, in Foucault's view, by the oppressiveness which the new view of human nature made possible.

This brief summary of the book's central thesis does not do justice to the brilliance of its exposition and the illuminative power of its *aperçus*. By traditional (or rather currently conventional) standards of scholarship (at least, those prevailing in this country) the book is a scandal, lacking in "original research" and making only the merest gesture toward modern scholarship in the field of penology. Needless to say, there is no bibliography or index, and the documentation is blithely free of the information other historians might find helpful in their researches. Like Foucault's other essays in "history of ideas"—such as *Histoire de la folie*, *Les mots et les choses*, *La naissance de la clinique*—this work also runs the risk of being consigned to that limbo category of "brilliant but unsound." To so judge it, however, would be wrong—or at least a "cate-

gory mistake." For it belongs to that genre of speculative essays of which Nietzsche's *Genealogy of Morals* is an example. It will remain seminal long after more conventional studies of criminality and penal institutions have been forgotten.

The book is divided into four sections, dealing successively with the subjects of torture, punishment, discipline, and imprisonment. Public torture, mutilation, and execution in the sixteenth century, Foucault argues, provided a kind of social ritual for the reassertion and re-establishment of the state's power over the subject. The removal of these activities to the secret domain of the dungeons during the late seventeenth and eighteenth centuries created an area of obscurity and ambiguity regarding the citizen's relationship to state power and generated a corresponding demand for the "clarification" of the law which the eighteenth-century reformers popularized. The organization of the modern state required both a clarification of that relationship and a reconceptualization of the purpose of punishment. The replacement of the concept of "subject" by that of "citizen" as well as the simultaneous demand of industrial society for a compliant and self-regulating citizenry were reflected in the reorganization of schools, military systems, hospitals, insane asylums, and prisons at the beginning of the nineteenth century. The aims of these new disciplinary institutions were the same, and all equally oppressive, Foucault alleges.

How would one confirm or disconfirm such a hypothesis? There is no way to do so, because, as Foucault's work always reminds us, what we mean by data and what shall serve as evidence are determined by the ends informing the discourse that one writes. Historians are not likely to take kindly to such an idea, given as they are to the notion that data are self-interpreting and evidence speaks for itself, if properly established. Foucault brings under question the criteria by which we establish what is "proper" and what is not. His is a remarkable achievement, significant especially for intellectual history, and *Surveiller et punir* is an important addition to his growing corpus.

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J. THORSTEN SELLIN. *Slavery and the Penal System*. New York: Elsevier. 1976. Pp. viii, 202. \$12.50.

J. Thorsten Sellin, a reputable criminologist, has written a useful book which may, unfortunately, be read only by those very few American historians actively engaged in penal history.

The basic thesis of *Slavery and the Penal System* was suggested by the German scholar Gustav Radbruch in 1938: "To this day, the criminal law bears the traits of its origin in slave punishments. . . . To

be punished means to be treated like a slave. . . ." Within a slim and tightly structured volume Sellin has validated this thesis by summarizing and relating definitions and applications of punishment (on a selective basis) from ancient Greece to the present. In each successive society—Rome, the Germanic kingdoms, France, Spain, England, Russia, and the United States—overall practices in administering punishment were similar. The "nobles" (in terms of wealth and power if not rank) were treated lightly (fines or exile) while "common men" were imprisoned at hard and/or degrading labor, mutilated or executed, as only slaves had been prior to the third century A.D.

Slavery survived longer in Russia and the United States than it did anywhere else in the West, and Sellin's best chapters compare Tsarist Siberia to Bolshevik Siberia and connect punishments imposed on black slaves prior to 1865 with punishments imposed on "free" blacks afterward.

Two crucial and related points are made in this book, both by implication because Sellin discusses neither. One is that while the West has "progressed" splendidly from the phalanx to the hydrogen bomb, we have made far less headway dealing with criminals. The other is that Sellin's findings are disturbingly applicable to the contemporary United States, in which thousands of poor, uneducated minority youths are annually jailed or imprisoned for stealing a few dollars while a felonious "ruler" is lavishly pensioned into "exile."

The book is not perfect. Whitelaw Reid's tour of the former Confederacy is described as having taken place during "1965-1966"; an administrative change within Louisiana's penal system which took effect after 1900 is dated from 1890; and Governor Jones of that state is referred to as Governor "James." There may be similar errors in chapters dealing with earlier periods.

Sellin has consulted many sources, primary and secondary, ancient and modern. His book is a solid addition to a skimpy collection of comparative penal histories.

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BASIL GREENHILL et al. *Archaeology of the Boat: A New Introductory Study*. Introduction by W. F. GRIMES. Middletown, Conn.: Wesleyan University Press. 1976. Pp. 320. \$16.95.

The early history of shipbuilding has long been a terra incognita. In the past decades it has been opened to exploration thanks largely to the new discipline of submarine archeology, which has brought to light the physical remains of ships ranging from a Bronze Age wreck off Asia Minor

to Sweden's celebrated Vasa. In this book Basil Greenhill, a noted specialist in the field, gives us an authoritative, well-written, and profusely illustrated summary of our present knowledge.

After a brisk review of the four fundamental devices man created to cross water—the skin boat, bark boat, raft, and dugout—and of their progeny, Greenhill comes to his major topic: planked boats and the two basic ways of building them. The earliest and for long the only method, and still the most widely used, is shell-first construction, in which the shipwright starts by creating a shell of planks, fastening each plank to its neighbors, and then inserts a stiffening of frames. The other is skeleton-first construction, in which the shipwright starts by setting up a skeleton of keel and frames and then clothes it with planks, fastening them to the frames; though a late development, it quickly produced the craft that enabled Europeans to conquer the seven seas.

For the ancient world Greenhill calls in the aid of John Morrison, who, in his necessarily compressed coverage does not emphasize sufficiently the uniqueness of two special practices of the Graeco-Roman shipwright, his use of multiple close-set joints to pin plank to plank and the strength of the framing he inserted. Greenhill devotes nine chapters to clinker-built vessels, including two on Viking craft to which Sean McGrail adds one on details not often treated, the Viking shipwrights' preference for green over seasoned timber and their method of fashioning planks (they favored oak, and they split the logs radially into slender wedges). Greenhill ends with a thoughtful chapter that points up, as no one has done so far, how very important was the introduction of the skeleton-first mode of construction; Greenhill calls it (p. 288), "one of the great technical achievements in the history of European . . . man." He admits that we cannot yet pin down the where and the when, but thinks we are closing in on the why: it was able to produce ships far larger than any shell-first craft and better able to meet the demands of long-distance oceangoing travel. Moreover, as Lynn White suggests (*Technology and Culture*, 16 [1975]: 530), they may have been far cheaper to build.

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JACQUES-BENIGNE BOSSUET. *Discourse on Universal History*. Edited by OREST RANUM. Translated by ELBORG FORSTER. (Classic European Historians.) Chicago: University of Chicago Press. 1976. Pp. xlvii, 376. \$25.00.

"History," wrote Bossuet to Pope Innocent XI, "is the supreme guide to life and politics," the study of

which demands "the greatest exactitude." When he published *The Discourse on Universal History* in 1681, Bossuet followed his own maxim by dividing the events of ancient history with the "greatest exactitude" into Epochs, Ages, and Empires. The Epochs marked the dialectical unfolding of a great event such as the "Fall of Troy," "Moses, or the Written Law," "Abraham, or the Beginning of God's People and the Covenant"; the Ages recorded the "Continuation of Religion . . . from the Creation till the Reign of Charlemagne"; and the Empires revealed to man how great states were overthrown because Providence ordained that princes be kept "in Humility."

Bossuet's transcendental tale has for several centuries excited critics to battle. In recent years, however, such scholars as Jacques Truchet, Thérèse Goyet, and Jacques Le Brun, in flight from polemic, have begun a re-examination of Bossuet's texts, placing them in the context of seventeenth-century rhetorical thought. Orest Ranum's and Elborg Forster's new edition and translation of the *Discourse* is a welcome offering to the Bossuet renaissance. Forster's translation admirably maintains the balance and rhythm of Bossuet's style without sacrificing the nuance of his thought. Ranum's introduction is a model of its kind: concise, insightful, and apt. As several of his modern commentators have noted, Bossuet was a rhetorician, a pedagogue who instructed the laity in the true meaning of past events, drawing moral lessons from historical portraits and tying the rise and fall of empires to the strengths and weaknesses of its people. As Leonard Krieger observes, Bossuet is a mediator "between the ancient and modern approaches to history."

JOHN C. RULE
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LUIGI AURIGEMMA. *Le signe zodiacal du Scorpion dans les traditions occidentales de l'Antiquité gréco-latine à la Renaissance*. (École des Hautes Études en Sciences Sociales, Centre de Recherches Historiques. Civilisations et Sociétés, number 54.) Paris: Mouton. 1976. Pp. 143. 125 fr.

This study of the evolution of the sign of Scorpio concentrates on four periods: late antiquity, the ninth through the twelfth centuries in Islam and Judaism, the Christian Middle Ages after the twelfth century, and the Renaissance. Luigi Aurigemma analyzes a number of texts from astrological and related documents and also examines images associated with such texts. The most valuable part of the book is its seventy full-page plates, four of them in color, many reproducing material accessible to few scholars in this country. Marginal notes key the plates to the text, and all but five of

them (nos. 5, 10, 27, 36, 51) have been reproduced well enough to be quite useful to the reader; unfortunately, for one who wishes to locate particular plates or to compare them, there is no table of illustrations.

Aurigemma recognizes that his selection of evidence is limited, and few will fault him for not studying every astrological item mentioned in the first six volumes of Thorndike's *History of Magic*. But there are difficulties in the choices he has made. His iconology might be more convincing if it took into consideration the Mesopotamian materials which influenced Greek and Roman treatments of the zodiac. Arab astrology, surely, is worth more than 1500 words, and medieval and Renaissance bestiaries deserve more than the brief nod he gives them. Certain figures played so great a role in the transmission of the ideas and images Aurigemma wants to understand that their absence from his study seriously diminishes its credibility: Arnaldus of Villanova, Ramon Lull, Antonio Guaineri, Marsilio Ficino (mentioned once, briefly), Pico della Mirandola, Gregor Reisch, Giordano Bruno, and others come to mind. On the other hand, Aurigemma is to be applauded for having examined a large number of manuscripts (over seventy in his bibliography) and for having gathered a respectable number of secondary sources covering a very long span of time (though specialists will find things wanting in this last regard as well).

Aurigemma's conclusions are difficult to summarize and nearly impossible for a historian to criticize. Briefly and too simply: his point of view is Jungian and he sees Scorpio as representing a dark side of the unconscious less well integrated into a psychic whole by medieval Christians than by their Greco-Roman forebears or their Renaissance successors. In so far as these views are susceptible to historical analysis, they rest on notions of ancient, medieval, and early modern history which are shallow, over-simple, and old-fashioned.

BRIAN P. COPENHAVER
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PETER J. BOWLER. *Fossils and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century*. New York: Science History Publications. 1976. Pp. viii, 191. \$9.95.

Recently, work on the history of nineteenth-century evolutionary biology has tended to focus more on questions of religion and philosophy than on basic hard scientific fact. Although this approach has paid rich dividends, the life-blood of the subject is, after all, real science. For this reason Peter Bowler's book, *Fossils and Progress*, one which promises to grapple with real problems in nine-

teenth-century paleontology and geology, is particularly to be welcomed. Moreover, it is a pleasure to report that Bowler fully keeps his promise. He has produced an absolutely first class little book in the history of science taken at its most fundamental level. By its very nature it will perhaps appeal more to the specialist than to the general reader; but few serious students of nineteenth-century thought can fail to read it with profit.

Briefly, Bowler's topic is the state of the fossil record: what exactly does it show? Bowler leads the reader from the beginning of the nineteenth century until it is about three-quarters over. At first, he shows how the fossil record could reasonably be seen either as progressive or non-progressive. Then the balance shifted towards progression. But not in an unequivocal way, for the Swiss-American Louis Agassiz saw evidence of a miraculously caused unilinear progression to man, whereas the Scot, Robert Chambers, saw an evolutionary unilinear progression to man. And then, just in time for Darwin's theory of natural selection, the fossil evidence was most reasonably seen as a branching tree, progressive, but not in some absolute sense.

Bowler claims quite convincingly that in the various interpretations of the fossil record, we see an aspect of the nineteenth-century conversion to evolutionism which depended essentially on basic facts, namely the ever-growing knowledge of the record as new fossils were uncovered. Whether it was quite as isolated as he implies I rather doubt. For instance, Bowler suggests that Owen's desire to see a progression to man was not really religious, but against this there is a letter from Owen to Whewell denouncing Chambers and putting himself on the side of those believing that "God created man in his own image, male and female"! But, all in all, Bowler's case seems persuasive. Let me repeat: this is a good book.

MICHAEL RUSE
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CARL VON CLAUSEWITZ. *On War*. Edited and Translated by MICHAEL HOWARD and PETER PARET. Commentary by BERNARD BRODIE. Princeton: Princeton University Press. 1976. Pp. xii, 717. \$18.50.

In presenting a new translation of Carl von Clausewitz's *On War* Michael Howard and Peter Paret have made a useful addition to both the study of that often-quoted but little-read classic and to military affairs in general.

Besides a new and readable translation, the editors have presented three essays and a guide which will be of major interest. The first, Peter Paret on the genesis of *On War*, is an all too brief con-

densation of his recent *Clausewitz and the State*. Paret offers a clear framework of development for Clausewitz's central concepts and their growth as he matured and experienced war, and is most helpful in placing Clausewitz firmly within the context of his own period. Limits of space make this essay seem somewhat rushed and disjointed in parts; interested readers will want to go on to Paret's book.

The second essay, Michael Howard on the influence of Clausewitz, is probably the most successful of the three. He deals with the effects of the classic in a coherent nation-by-nation treatment, allowing the reader to comprehend developments of thought within separate military groups over the years.

Bernard Brodie's essay, on the continuing relevance of *On War*, is the least successful of the introductory essays. Though pointing out some of Clausewitz's still trenchant observations, this essay often seems to be more of a pep talk for doubtful readers seeking incentives to goad them on to read a difficult work.

Brodie redeems himself, however, with his guide at the end of the volume. Although this section veers between analytical guide and synopsis most readers will find it helpful. Moreover, in providing cross references to concepts which appear in different parts of *On War* and in updating Clausewitz's eighteenth-century examples with more vivid and recognizable modern ones Brodie has done the reader a great service.

Finally, the translation itself reads well and avoids most stilted constructions. It is a welcome improvement. Although the essays and text are footnoted, the absence of a bibliography is a major flaw in the work. Clearly the editors intended to produce more than just a new translation, and a bibliography such as that in Paret's own book would have been most appropriate.

MARK M. LOWENTHAL
Library of Congress

JACK C. ROSS. *An Assembly of Good Fellows: Voluntary Associations in History*. Westport, Conn.: Greenwood Press, 1976. Pp. xiii, 325. \$14.95.

Characterized by its author as historical sociology, this book attempts to account for the prevalence and existence, or nonexistence, of voluntary associations in five primitive societies, five ancient civilizations (Judaic, Greek, Roman, Indian, and Chinese), and preindustrial England. The discussion of English voluntary associations takes up approximately half of the book and deals chiefly with guilds and clubs. The analytic tool is a three-part model having social, cultural, and personal levels. The first is subdivided into communication,

connectivity, permanence, and permission; the second into goals and interests; the third into resources and rewards. Jack C. Ross argues that the prevalence of voluntary associations in a society is not related to class but correlates with the degree to which these analytical categories exist. A work of synthesis rather than of original research, the book draws most of its materials from encyclopedias and the writings of anthropologists, sociologists, and historians.

Ross complains that the terminology of other disciplines is neither technical nor abstract enough to come readily to the mind of a sociologist seeking bibliography for interdisciplinary research. Perhaps this explains the strange omissions and substantial errors of fact. For example, had Ross consulted E. M. Carus-Wilson's definitive book on medieval merchant venturers, he would not have described those exporters of woolen cloth as "import specialists." Notable eighteenth-century omissions include the invention and development of the associational philanthropic corporation and the Charity School movement. Such oversights not only distort the picture of English voluntarism but also cast doubts upon the validity of conclusions that ignore them.

A thicket of sociological jargon, soporific verbosity, irrelevancies, grammatical errors, and textbook type organization make for a singularly graceless style. Too often there occur such sentences as: "one of the basic concerns of the gild was the regulation of problems of interpersonal relations, caused in part by the drinking customs of the times" (p. 209)—twenty-six words to say muddily what can be said in five: guilds patched up drunken quarrels. Whether the book is good sociology I do not presume to judge, but to the historian this offspring of a marriage of disciplines must seem disappointing.

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J. BOWYER BELL. *On Revolt: Strategies of National Liberation*. Cambridge, Mass.: Harvard University Press, 1976. Pp. xv, 272. \$15.00.

Having interviewed several hundred participants in nationalist revolts against Britain since World War II, J. Bowyer Bell has prepared an intriguing medley of historical narrative, abstract theory, and practical strategic counsel for rebels—and for the imperial "center" they defy. Of the eight cases narrated—at one moment as if by a dispassionate analyst on the nationalist general staff, at the next as if by a nonpartisan observer attached to rebel headquarters—the stories of the Irgun, EOKA, and the South Arabian NLF are most useful in illuminating the inner workings of movements un-

til now mostly observed from a distance or with *parti pris*.

From his case studies, Bell reaches the persuasive conclusion that, during this postwar era, the British center truly was the same opponent to all its rebels—a gradually-declining empire with a faster-decaying rationale, committed to a devolution whose pace and direction barely satisfied some subject groups, and utterly frustrated others. Britain, he finds, was in these circumstances unwilling, or psychologically unable, to use efficiently and wholeheartedly the full force at its command to crush rebellion, unless the rebels (like the Mau Mau and the Malayan Communist Party) through their own conduct quite shattered their legitimacy in *British eyes*. Generalizing more broadly, Bell argues that nationalist rebels surprisingly often give gratuitous aid and comfort to imperial counterinsurgency by overlooking the peculiar and settled tendencies in the manner of response of their imperial center to different modes of resistance. Such rebels may blindly provoke the center to take effective counter measures even while other rebels against the same empire, armed with a more objective analysis, contrive to stimulate in their masters only a profound desire to depart.

As the author claims, these cases offer a wealth of lessons. But Bell's editors have been too indulgently forbearing—in small matters (a passing reference to a published "photograph of Mohammed," p. 6) and in large. The narratives are lucid and straightforward; but students of the anatomy of revolt should be prepared to expend considerable effort in excavating the steaming heaps of teasing, rambling, plummy prose in which the dismembered insights of Bell's analysis lie buried. By and large they will find the effort worth the inconvenience.

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ANCIENT

RUDOLF PFEIFFER. *History of Classical Scholarship from 1300 to 1850*. New York: Oxford University Press. 1976. Pp. ix, 213. \$16.50.

This second and concluding volume of Rudolf Pfeiffer's survey of classical philology makes a significant contribution to early modern intellectual history. The book opens with the great humanist scholars of fourteenth- and fifteenth-century Italy and culminates in the fresh direction given the discipline by Johann Winckelmann in nineteenth-

century Germany. The author has provided an incisive, thoroughly researched and, on occasion, moving account of the generations of patient, often brilliant classicists who have, since Petrarch, sought to recover, interpret, and make available to the Western world the literary treasures of Greek and Roman antiquity.

Pfeiffer distinguishes three major phases in the history of classical philology between 1300 and 1850. He begins with the impetus given Latin studies by Petrarch and his successors in Italy, and advanced by Northern humanists such as Erasmus and Joseph Scaliger. This Renaissance era scholarship also became deeply concerned with what Pfeiffer calls the "religious problem," evidenced in the Biblical editions and commentaries of Erasmus.

Pfeiffer sees these interests persisting throughout the seventeenth century, yielding only gradually in the eighteenth to a decisive new cultural force, a "Neohellenic" revival that drew its prime inspiration from Winckelmann. And beyond the shift of interest after 1700 from Latin to Greek studies and from Italy to Germany as the most fertile source of the new direction was the virtual eclipse of the religious concerns that had so frequently engaged the earlier humanist-scholars.

Finally, after 1850, humanism itself ceased in Pfeiffer's judgment to be the driving force in philology, displaced by currents of realism and historicism as most obviously embodied in the approach of Theodor Mommsen. But it is humanistic philology in its Renaissance and Neohellenist manifestations that most intrigues Pfeiffer. He pursues the modern history of the subject no further.

Transcending these phases of early modern philology, however, is the abiding reality of what Pfeiffer terms the *philologia perennis*, the essential continuity of the great themes that have characterized classical scholarship from its inception among the Hellenistic scholar-poets of the third century B.C.

Because of advanced age the author abandoned his original plan of including the thirteen centuries from Caesar Augustus to Petrarch. The resulting gap means a foreshortened perspective on the early modern developments and impairs somewhat Pfeiffer's leitmotif of the continuity in classical scholarship. There is also a regrettable reluctance at times to elaborate sufficiently on revisionist positions, as in the discussion on Petrarch.

Yet what Pfeiffer has done clearly supersedes John Sandys' standard work (1903-08). Concise footnote summaries of current scholarship and stimulating observations about areas where profitable work remains to be done will be particularly appreciated by all students of the subject.

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RICHARD STILLWELL *et al.*, editors. *The Princeton Encyclopedia of Classical Sites*. Princeton: Princeton University Press. 1976. Pp. xxii, 1019. \$125.00.

Consisting of roughly three thousand entries written by an international group of 375 scholars, this massive work of reference generally fulfills its aim of providing a one-volume compendium of information on classical archeological sites. Its chronological and geographical span is extensive, comprising the mid-eighth century B.C. to the beginning of the sixth century A.D. and encompassing the Greco-Roman world in its broadest sense, from Britain to the Indus and beyond. Each entry consists of a brief historical sketch of the site in the classical period followed by a description of the remains, a note on the museum(s) where the finds are located, and a bibliography. The omission of site plans is an unfortunate editorial decision which robs the volume of considerable value as a research tool.

As is perhaps inevitable in a work of this sort, the product of cooperative effort, the quality of the individual articles is highly uneven. Many of the longer articles are significant contributions; other entries offer nothing more than a thumbnail survey of the history of the site and the barest mention of the remains. Particularly valuable for the historian is the inclusion of large numbers of minor but historically significant sites. There are also some egregious lacunae in the list of sites treated. The reader looks in vain, for example, for entries on such major sites as Pyrgi and At Khanoum, excavations which for the historian must rank among the most important and exciting postwar developments in classical archeology.

It is the bibliographies, however, which are the weakest feature of the *Princeton Encyclopedia of Classical Sites*. Only in a very few cases is reference made to works published later than 1973. Perhaps the printing lag for such a complex volume explains the 1973 cut-off date. More disturbing is the impression that even in regard to the earlier literature there are major gaps in the bibliographies appended to the individual entries. Examining the articles on those sites with which he is particularly familiar, this reviewer noted a number of surprising bibliographical omissions. Thus, for example, the article on Cumae takes no cognizance of a series of important recent studies, all published by 1973 (M. Napoli in *Atti del quarto convegno di studi sulla Magna Grecia* [1964]; J. Christern in *Mitteilungen des Deutschen Archäologischen Instituts, Römische Abteilung* [1966/7]; M. Bertoldi, *Bolletino d'Arte* [1973]; A. McKay, *Vergilius* [1973]). The article on Alexandrian foundations was written in apparent ignorance of the major general works (such as Tarn, *Alexander the Great* [1950] II, and *The Greeks in*

Bactria and India [1951], and V. Tscherikower, *Die hellenistischen Städtegründung von Alexander bis auf die Römerzeit* [1927]) and of various treatments of individual foundations (such as A. Foucher in *Comptes rendus de l'Académie des inscriptions et belles lettres* [1939] on Nikaia in Afghanistan and G. Radet in *Revue des études anciennes* [1941] on Buchephala and Nikaia on the Jhelum). These are not isolated examples. At times the bibliographical omissions border on the ludicrous. The article on Alexandria, for example, makes no reference to Fraser's monumental *Ptolemaic Alexandria* (1972). In light of the prior existence of such extensive reference works as the *Real-encyclopädie der classischen Altertumswissenschaft*, *Der Kleine Pauly*, and the *Enciclopedia dell'arte antica, classica, e orientale*, a primary justification for the production of so expensive a volume as the *Princeton Encyclopedia* lies in providing the reader with ready access to the most recent publications on the individual sites. It is to be regretted that this lack of bibliographical completeness detracts from what is otherwise an extremely useful handbook.

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VASSOS KARAGEORGHIS. *View from the Bronze Age: Mycenaean and Phoenician Discoveries at Kition*. New York: E. P. Dutton and Company. 1976. Pp. 184. \$19.95.

Vassos Karageorghis, the distinguished Director of Antiquities of Cyprus, gives us here an account of his work at Kition (modern Larnaca) on the south-eastern coast of Cyprus. The book covers more material and says less than either its title or subtitle would suggest. Major chapters describe the cemeteries of the Early Bronze Age, material from the Late Cypriot IIB (1300–1230 B.C.) period showing the characteristic interchange of Aegean and Near Eastern influences on Cyprus including Levantine and Anatolian mixed with Minoan and Mycenaean contacts, the Late Cypriot IIIA (1230–1190) and IIIB-C (1190–1050) periods dominated by two waves of Mycenaean settlement and finally, after an abandonment of 150 years, the major Phoenician settlement lasting until its destruction by Ptolemy I (ca. 850–312 B.C.).

The primary finds are concentrated in two areas, both extremely limited in size. Area I has tombs and houses. Area II contains part of the city wall, temples, sacred areas, and industrial shops. The main Astarte temple is the largest Phoenician temple yet known. Most interesting is the evidence for "sacred gardens" and "sacred barbers." These very important finds affect our knowledge of a number of aspects of history and religion.

This is a handy summary with excellent illustrations of material previously available only in separate journals and volume one of the excavation report. Until the final reports are complete, years from now, the archeologist will find it disturbing to have material indicated in the illustrations that is not discussed in the text.

The historian will have more severe problems. The text is mostly descriptive and hard to follow because of its compression of material and unexplained archeological terminology. The short statements on the significance and interpretation of the material have great bearing on biblical and religious history as well as Mediterranean interrelations. Many of the points, however, are made too quickly and too sparsely to be convincing without recourse to other publications. The book is too technical at some points and too skimpy at others for the general historian. In short, it gives neither archeologist nor historian enough material to do justice to the author and his excavation.

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LIDIA STORONI MAZZOLANI. *Empire without End*. Foreword by MARIO PEI. Translated by JOAN MCCONNELL and MARIO PEI. New York: Harcourt Brace Jovanovich. 1976. Pp. xxxi, 242. \$10.95.

The author will be known to many readers by her previous book, *The Idea of the City in Roman Thought*. The present volume fits into the same category of philosophy of history. She undertakes to trace the views of the three great Roman historians of the late republic and early empire, Sallust, Livy, and Tacitus, toward the Roman state and the sense of seemingly inevitable decline that tormented thinking Romans after Rome's heyday (whenever one considered that was). Her title is taken from Vergil's *Aeneid*, where Jupiter in Book I promises Venus that he has given Aeneas' people *imperium sine fine*. But the burden of the book is the response of three major figures to the realization that the empire was indeed faced with an end; this is conveyed by the subtitle, which, however, appears only on the dust jacket and nowhere in the volume itself. This absence on the title page may well mislead a reader.

This is not a book for the novice or for one whose knowledge of Roman history is superficial. Lidia Storoni Mazzolani produces an effect greater than the sum of the various parts; she is more successful as philosopher than as historian. Her discussions of Sallust on harmony (*concordia*), Livy on virtue (*virtus*), and Tacitus on power (*dominatio, potentia*) are largely persuasive. Yet her argument leaves much to be desired; she places emphasis on much that is wrong, such as the importance of the patri-

cians in her study of Sallust when it is the nobles, not patricians, who control the state. This is terribly misleading, and numerous other errors will not evoke confidence. Statements—such as “Sulla's Asian campaigns, which ended in 62 B.C.” (p. 46) and (about the Piso who was Germanicus' enemy) “Lucius Calpurnius Piso schemed, with Seneca and Lucan, to replace Nero as emperor and was executed in A.D. 70” (p. 185)—make one wince.

Mario Pei has written a long and tendentious foreword which begs the question, “Can we use the past as a guide for the present?” Yes, of course we can, but . . . ; for a sane discussion of this problem see M. Hammond, “Ancient Rome and Modern America Reconsidered,” *Proc. Mass. Hist. Soc.*, 73 (1961, publ. 1963): 3–17. Pei too is subject to imprecision in his treatment of “patricians” and other concepts; hence the entire book tends to mislead, and the factual errors are dangerous. The translation is elegantly done, but the scrutiny of a trained historian has not been obtained. Caution in use must therefore be the verdict on a volume that could so easily have merited warm approval.

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RICHARD DUNCAN-JONES. *The Economy of the Roman Empire: Quantitative Studies*. London: Cambridge University Press. 1974. Pp. xvi, 396. \$22.50.

This book is not a general economic history, but a collection of seven very useful studies in which Richard Duncan-Jones examines the sources of wealth in the western part of the Roman Empire and discusses the economic context and social applications of this wealth. The author maintains that these questions cannot be properly studied without quantification, a term which might suggest the use of much more evidence than actually exists. Ancient sources for such a study are not plentiful, but the author has carefully assembled those available and has scrutinized them carefully. Chapters 1, 3, 4, and 7 of this book appeared in an earlier form in the *Papers of the British School at Rome*, and a part of chapter 6 in *The Journal of Roman Studies*, but the earlier studies have all been substantially revised.

The finances of Pliny the Younger are the subject of the first chapter. As would be expected, most of Pliny's wealth derived from landed estates. Pliny is the only senator of the Principate about whose finances we have sufficient data to make possible such a study. His many generosityes are known from his Letters, and from a posthumous inscription that records Pliny's gifts to his native town of Comum.

In the second chapter, agriculture—the basic

source of wealth in the Roman Empire—and the rates of profit from it are examined. The preserved evidence is in the agricultural writers. The only crop for which there is explicit evidence for rates of profit is wine. Columella gives specific figures for profit, but Duncan-Jones convincingly argues that these are inaccurate, for Columella fails to include a considerable number of capital outlays as well as overhead expenditures in his calculations. Columella's minimum acceptable yield of three cullei (15.72 hectoliters) per iugerum (2,546 m²) is also questioned by Duncan-Jones. He believes that both this yield and the wine prices given reflect Columella's experience on his estates but did not hold for all of Italy. He compares Columella's minimum acceptable yield with modern Italian wine yields quoting (without explaining why) the average yield for 1909–13 (1.16 cullei per iugerum) and for 1909–36 (1.17 cullei per iugerum), and claims that Columella's yield is nearly three times greater than the yield in modern Italy. An examination of modern yields, however, shows a great range, and yields can be cited that are comparable to Columella's minimum yield, as for example the average yield from 1971–74. Any comparison with modern yields is at best difficult. Columella's figures, however, even as corrected by Duncan-Jones leave the impression that viticulture was the most profitable of the major forms of cultivation although certain luxury items near large cities undoubtedly brought higher returns.

Money and prices are next considered (chapters 3 and 4). The evidence is primarily epigraphical, and it is preserved for the most part in Italy and in Africa Proconsularis including Numidia (which later became a separate province). The African inscriptions contain valuable information about the *summae honorariae* (the fixed charges for local office), also considerable information about the prices of buildings, statues, tombs, and funeral monuments. African inscriptions also record a sizable number of perpetual foundations, as well as prices for games, feasts and distributions. Most of the inscriptions which list these costs appear to belong to the period A.D. 98–244. They are conveniently tabulated in the classified table at the end of chapter 3. In each case the cost is identified, the price given in HS, together with the town, the date of the inscription (if known), and the reference for the inscription. Over 464 costs are tabulated. Italy provides about twice as many inscriptions (893). These contain few prices for public buildings and statues, but many more prices for foundations, *sportulae*, and tombs. Neither area furnishes much information about commodity prices. In chapter 5 the author examines the use of prices in the Latin novel and shows that these prices have little reliability for the economic historian.

Because of the importance of the availability of manpower in an agrarian society, the sizes of cities and of town organizations are discussed in chapter 6. By applying more broadly the technique first used by Beloch of arriving at the size of town populations from the information about large-scale gifts for feasts or distributions, Duncan-Jones shows that the previously estimated sizes of some cities is probably too large. In the final chapter he examines government subsidies to encourage population increase (the *alimenta*).

In addition to the lengthy tables at the end of the chapters, there are seventeen appendixes, some in tabular form. These document the subjects discussed in the book and provide a valuable reference tool for scholars working on many diverse problems. This book makes a significant contribution to the economic history of the Roman Empire.

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STEPHEN JOHNSON. *The Roman Forts of the Saxon Shore*. New York: St. Martin's Press. 1976. Pp. ix, 172. \$18.95.

The author's assertion that the Saxon Shore is a "neglected corner of the Roman defensive system" is a bit misleading. The forts in France, Belgium, and southeastern England that monitored passage through the Channel are certainly less well known than the stations on Hadrian's Wall, but they have not been ignored. Indeed, a monograph of similar proportion and design was published by D. A. White in 1961 (*Litus Saxonicum*), and a number of reports and articles on individual forts, especially those in England, have appeared more recently. The chief virtue, and justification, of Stephen Johnson's book is that it provides an up-to-date survey of the archeological evidence, refutes several of White's interpretations, and reassesses the anomalous entries in the *Notitia Dignitatum*, which documents the administration of the Roman Empire in the late fourth and early fifth centuries.

The author insists that "only by appreciating this series of defences as a frontier system can we judge its working." Whether the Saxon Shore was in fact a "frontier," however, is debatable. Johnson's equation of *limes* (a network of forts on the perimeter of Roman territory) and frontier (the territory within and beyond the official boundaries of the Empire) does not inspire confidence in his conceptualization. Nor is it certain that an investigation of the Saxon Shore can serve as "a model for the study of other frontiers within [*sic*] the Empire." The Saxon Shore, as Johnson points out, was the only maritime "frontier;" the forts in the Julian Alps also monitored established trade

and migration routes, but they manifestly were not maritime installations.

Johnson is at his best when describing the forts themselves or connecting excavated sites with entries in the *Notitia*. His conclusion that the Saxon Shore section of the *Notitia* was compiled between A.D. 367–69 and 395 is generally persuasive. By this time the command had been divided: the continental forts were supervised by two *duces*, while the British garrisons remained under the jurisdiction of the *comes Litoris Saxonici*. Equally convincing is Johnson's argument (against White) that Carausius, initially the commander of the channel fleet and later a rebel, was not the architect of the Saxon Shore system. The archeological evidence clearly indicates that most of the British forts antedate Carausius' command. These forts survived the suppression of his rebellion and were still occupied in the last quarter of the fourth century. Only Richborough, however, remained in use after the withdrawal of the Roman troops during the first decade of the fifth century.

The wealth of new data and the author's assessment of earlier hypotheses make this an eminently useful book, whatever one thinks of the basic formulation of the problem. It should be read by all students of Roman Britain.

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RAMSAY MACMULLEN. *Roman Government's Response to Crisis, A.D. 235–337*. New Haven: Yale University Press. 1976. Pp. ix, 308. \$17.50.

Ramsay MacMullen continues to demonstrate his evergrowing stature as a historian of Roman society under the Empire by the continuing procession of his books and many articles. He has an enviable talent for frequently seeing well-known data in an entirely new way; thus he educates and sharpens the perceptions of his fellow historians of ancient Rome. He makes the work of Samuel Dill, dependent almost entirely on literary sources, seem to belong to the eighteenth century, although in fact Dill's books were published only a generation or so ago and are still interesting and readable. MacMullen examines not only literary sources, but every sort of archeological trace of later Greco-Roman antiquity.

The present book is characteristic of MacMullen's work: it has no particular "thesis," but considers a number of topics connected with its subject, while assembling the evidence and modern argumentation and looking at everything with "new eyes." The result is that its readers, even lecturers to freshmen in a survey course, will have to change a good many things in their notes. In

fact, MacMullen has written eight essays, followed by a brief summary, on his subject.

The headings are as follows: "The Perception of Decline" (the Christians saw universal decline when they were being mistreated; in similar circumstances the pagans merely saw particular difficulties); "Propaganda" (the Empire's people saw the crisis as a moral one [not new], and imperial propaganda responded by emphasizing the moral, and sometimes supernatural, virtues centered on the Emperor, as the means to combat it); "Intelligence" (the humanistic education of the ruling classes was incompetent to meet the crisis; above all, its statistical skills, what MacMullen calls "numeracy," were totally inadequate); "Law" (which became an affair of bureaucrats after the great age of "classical" jurisconsults came to an end); "Money" (the expenditures of the government, military above all; this will be the most controversial chapter, for few scholars will approve it given the problems of reconciling numismatic and literary evidence); "Taxes" (almost as difficult as the former, as MacMullen tries to present the problems of the system of adding units of labor and land—adding oranges and apples); "Goods and Services" (compulsion was not so much a departure as sometimes thought, for in the past sons had usually followed their fathers' occupations; even the Emperor was bound to his position, "*obnoxius necessitati*" [p. 170]); "Defense" (new aspects of the well-known military needs of the government).

One must complain, however, of the lack of a general bibliography, which compels the user for reference purposes to hunt through the text and accompanying notes. And the title is somewhat misleading; the book in fact deals with the interaction of subjects and government in response to the crisis.

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LUKAS DE BLOIS. *The Policy of the Emperor Gallienus*. (Studies of the Dutch Archaeological and Historical Society.) Leiden: E. J. Brill. 1976. Pp. xi, 242. f 72.00.

The emperor Gallienus has long been surrounded by scholarly controversy, so it is perhaps surprising that this should be the first full-scale modern work devoted entirely to him. The book sets out "to investigate the background and the aims of the policy of Gallienus and the significance of his more important measures" (p. ix). In pursuance of this objective the book is divided as follows: 1.) a brief narrative account of the period 253–68 followed by a brief survey of some of the social, economic, and administrative features of the period; 2.) a dis-

cussion of Gallienus' military, administrative, and financial policy; 3.) an analysis of Gallienus' conception of the emperorship; 4.) a discussion of Gallienus' religious policy; 5.) Gallienus' cultural policy.

Each of these themes is divided into a number of subthemes, which makes for clarity of exposition and easy reference. It is a pity, though, that Lukas de Blois has set his sights as narrowly as he has. The focus remains fixed on the period of Gallienus' sole rule, 259 to 268, and there is hardly any mention of events before or after this brief decade. This lack of contextual background is all the more disappointing, since one of the most important questions to be asked about Gallienus is: how typical was he of third-century emperors?

Another disappointment is the lack of any discussion of Gallienus' family background, and in particular of his father Valerian, the only Roman emperor ever to be captured in battle. Not only would the relationship between Gallienus and his father have provided a "human interest" story, but it may also have given us some insight into the marked differences in policy between father and son—though not necessarily a Freudian interpretation of them!

Despite these omissions, de Blois' conclusions are commendably judicious, essentially falling midway between the ancient picture of Gallienus as a weak and ineffectual ruler and the fashionable modern view which portrays him as an original thinker and innovator. Gallienus emerges from this study as a fairly typical military emperor of the third century, cast in much the same mold as his successors Claudius Gothicus, Aurelian, and Probus, and in this sense a forerunner of Diocletian. But, on the basis of the evidence presented to us here, we may equally well discern in him an heir to the long line of antisenatorial emperors going back to Gaius, Nero, Domitian, and Septimius Severus.

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Dumbarton Oaks Papers, number 29. Washington, D.C.: Dumbarton Oaks Center for Byzantine Studies. 1975. Pp. xiii, 338.

As is customary with the series, a major portion of this volume is devoted to the proceedings of the 1974 Dumbarton Oaks Symposium on "The Decline of Byzantine Civilization in Asia Minor, Eleventh-Fifteenth Century," with papers by Peter Charanis, Helene Ahrweiler, Speros Vryonis, Nicole Thierry, and Anthony Bryer included here. This was one of the most successful of the symposia, for it brought together scholarship in an

area in which major advances have been made in recent years.

The social transformation of the countryside in these centuries was fundamental and long-lasting, as the pattern of cities and settled agriculture characteristic of ancient and Byzantine society gave way to the nomadic transhumance which has prevailed in Asia Minor down to modern times. Vryonis describes the processes of nomadization and Islamicization, contrasting, (as he has done at greater length in *The Decline of Medieval Hellenism in Asia Minor*) the destructiveness of the nomadic incursions with the successful absorption of the Christian population by settled Islamic states. Why did Byzantine resistance fail so completely? Ahrweiler suggests an answer in her analysis of the experience of the Empire of Nicea. Contrary to other recent students of Nicea, she believes that the experiment was doomed from the start by the premises of its foundation. For the exiles who fought their way into the city after the fall of Constantinople in 1204, Nicea was always a mere basis for reconquest of the capital; the mentality of "*un an prochain à Constantinople*" dominated. But, Ahrweiler argues, the indigenous population who had welcomed the Latins as liberators and resisted the entry of the exiles represented a political ideology based on the needs and interests of the provinces, bitterly opposed to court imperialism. If John Vatatzes and Theodore II Lascaris followed a more "Nicean" policy, with reconquest a less imminent goal, the victory of Michael Paleologus signalled the final sacrifice of provincial needs to imperial policies. The antagonism between court aristocrats and provincial citizens, which Ahrweiler has traced as a major theme in Byzantine political ideology, is more easily and persuasively traced in court works than in documents which may be identified as provincial. Judith Herrin's account of Byzantine provincial government in Hellas and the Peloponnesos between 1180 and 1205 ("Realities of Byzantine Provincial Government: Hellas and Peloponnesos, 1180-1205") reinforces the point. Whereas church officials in the provinces were capable, Constantinople-trained men who took the problems of their provincial church seriously, military and civil administrative chiefs remained in Constantinople, or used provincial assignments as a source of personal enrichment.

In the most striking paper of the symposium, however, Anthony Bryer clearly demonstrates that Byzantine failure before the nomads was neither necessary nor universal. His careful study on the "Pontic Exception" analyzes the success of the empire of Trebizond in containing and coexisting with the Turkmen. Isolated, and lacking ambitions in Constantinople, the Grand Comnenoi

were able to use traditional means of marriage alliance and diplomacy to keep their valleys intact. Throughout the thirteenth and fourteenth centuries, the people of Hatt developed a symbiosis with the Turkmens which survived the eventual Ottoman conquest of Trebizond in the fifteenth century. Bryer's analysis, based on a careful interpretation of the text of Panaretos and the use of Turkish heroic poetry, is an impressive example of the possibilities of regional history in Asia Minor.

The other studies in this volume will be of most interest to art historians. They include Nichole Thierry's "L'art monumental byzantine en Asie Mineure du XI siècle au XIV," Shigebumi Tsugi, "The Headpiece Miniatures and Genealogy Pictures in Paris Gr. 74," Lawrence Nees, "An Illuminated Byzantine Psalter at Harvard University," Ioli Kalavrezou-Maxeiner, "The Imperial Chamber at Luxor," Jacques Ryckmans, "The Pre-Islamic South Arabian Bronze Horse in the Dumbarton Oaks Collection," as well as reports on excavations at Kalanderhane Camii in Istanbul and Dibsi Faraz, Northern Syria, and notes on five seals of *oikistikoi* (John Nesbitt), the Cyprus plates and the Chronicle of Fredegar (Stephen Wander), and the Praetorium at Musmiye (Stephen Hill).

DOROTHY DE F. ABRAHAMSE
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thus their historical visions and methodologies. Of paramount importance were their attitudes toward the Christianization of the Empire. The evolution of perspective on the struggle between paganism and Christianity is expertly demonstrated through a chronological survey beginning with Ammianus' patently anti-Christian view and extending through Menander's and Theophylact's transcendent reconciliation of the two religious outlooks as the Empire as a whole assimilated and triumphed over paganism. As the author deftly demonstrates, this elite group of historians provides elegant testimony not only to the tensions and development in early Byzantium, but also to an enduring commitment to the classical mode of historical inquiry. Rather than adopt the new, uncritical universalism of the chronicler-monastic, they clung tenaciously to the mastery of evidence, the use of eyewitness testimony, the specific chronological focus, and the rhetorical precision which constituted the esthetic of historical writing in the older Rome and in the Athens before her.

Historians of Byzantium and historians of the history of history owe a great debt to this imaginative and well-written study of a heretofore neglected school of historians.

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Z. V. UDAL'TSOVA. *Idenio-politicheskaia bor'ba v rannei Vizantii (po dannym istorikov IV-VII vv.)* [The Ideological and Political Struggle in Early Byzantium According to the Data of Historians of the Fourth through the Seventh Centuries]. Moscow: Izdatel'stvo "Nauka." 1974. Pp. 330. 1 r. 76 k.

In this excellent monograph Z. V. Udal'tsova examines the historical writings of a number of secular historians who experienced and wrote about the earliest period of Byzantine history. She addresses herself primarily to the works of Ammianus Marcellinus, Eunapius, Olympiodorus, Zosimus, Procopius, Agathias, Menander Protector, Theophylact Simocatta, and Pseudo Mauricius. She classifies all of these writers as members of a historiographical school dedicated to the conservation of the style, the methodology, and the values of the great historians of classical antiquity. Deeply committed to the preservation of an elitist and aristocratic way of life, they participated in and wrote about Byzantium's struggle to reconcile her pagan background with the foundation of a new, Christian, and very imperial Rome.

Udal'tsova considers each of the historians biographically in order to appreciate the experiences which informed their intellectual attitudes and

MEDIEVAL

DENO JOHN GEANAKOPOLOS. *Interaction of the "Sibling" Byzantine and Western Cultures in the Middle Ages and Italian Renaissance (330-1600)*. New Haven: Yale University Press. 1976. Pp. xxii, 416. \$27.50.

This important study sheds light upon the ways in which Western Latin culture and Byzantine Greek culture interacted over a period of some thirteen hundred years. It consists of two parts. The first deals with this interaction or acculturation down to the end of the thirteenth century—that is, during the medieval period. The second is concerned with contacts during the Renaissance era. The author further divides these years into subperiods: from A.D. 330 to 1095, the time of the First Crusade; from A.D. 1095 to 1261, when the Byzantines recovered Constantinople from the Latins; from 1261 to 1453, when Constantinople was captured by the Ottoman Turks; and from 1453 to 1600, by which time the Greek Diaspora was over and its artists and scholars had passed on to the early modern West the most significant elements of their unique civilization. Each of these subperiods seems to Deno John Geanakoplos to possess distinctive

characteristics which very much affected the ways in which these two cultures interacted.

Though other cultural elements are not neglected, the author, throughout this study, concentrates his attention upon the unique character of the Orthodox Church and the differences between it and the Latin Church which repeatedly hindered fruitful cooperation between these civilizations. He also stresses the role of art and architecture, of intellectual and philosophical concepts in the process of cultural interchange. Especially interesting is his insistence, correct in the eyes of this reviewer, that Western Thomistic scholastic thought had a real impact upon Greek intellectuals during the Paleologue period, that much of the spirit and many of the techniques of quattrocento Italian art were anticipated or paralleled in the Byzantine world, and that there was a unique, special kind of Paleologue Renaissance which explains how Byzantine culture was passed on to fifteenth- and sixteenth-century Western Europe.

It is important, however, for a Western-oriented scholar to point out some ways in which this useful, pioneering synthesis might have been improved. In general, especially when dealing with the early Middle Ages, the author paints much too dismal a picture of the culture of the Latin West. After all, Carolingian, Anglo-Saxon, and Ottonian Europe was able to produce a viable art and architecture and some culture of consequence. And there is a need, in bibliography and text alike, for the author to consider cultural interaction taking place in Western Europe itself instead of concentrating so exclusively on the East—at least down to A.D. 1400. These caveats aside, Geanakoplos is to be congratulated for dealing in a forthright, lucid, and challenging manner with a subject which should engage the interest of a wide circle of historians.

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GUY FOURQUIN. *Lordship and Feudalism in the Middle Ages*. Translated by IRIS SELLS and A. L. INTTON SELLS. New York: Pica Press. 1976. Pp. 253. \$12.50.

In 1970 the Presses Universitaires de France published this book as part of its series, *Les précis de l'enseignement supérieur*; as this suggests, it is intended to be one of the pieces of scholarly popularization which, at their best, the French do superbly well. Confirming this goal, the jacket of the American edition of the slim volume promises "new light upon a dark aspect in the social history of Europe from the mid-ninth to the fourteenth and fifteenth centuries." There can be no quarrel

with the aims of either author or series, but the book Guy Fourquin has written hardly fulfills them, and students and lay readers should beware of both the French edition and the English translation of the book.

As the title indicates, the medieval past is here viewed through the prism of the eighteenth- and nineteenth-century historical categorizers, and one of Fourquin's acknowledged purposes in writing the book was indeed to correct "the old confusion between fuedalism and lordship" (p. 12). His focus thus uncertainly shifts between defining abstractions and describing the realities of medieval social life, and the limitations of space imposed on him do not permit him to deal satisfactorily with either subject. He is forced to assume knowledge of the fundamentally important and quite complex conditions existing in the middle of the ninth century; in the interests of economy he repeatedly resorts to metaphor and personification. The shadow of Ganshof is omnipresent, and if, from time to time his influence is displaced by that of Bloch, Duby, and Genicot, their methodologies and findings sit uneasily within the analytical framework derived from Ganshof's work. Further discussion of the substance of the book might well appear redundant, for in a sense, I have already reviewed this book in criticizing others that are much like it.

Still, a word should be said about the American edition of the book. The translators readily admit the problems they encountered, calling the book "an exceptionally difficult work to put into English" (p. 7). They can take comfort from the fact that Fourquin's abbreviated abstractions and use of technical legal terms cannot possibly be any easier for the French amateur than for the American. But this was by no means the translators' chief problem. Although the Sells are experts in English and continental literature of the seventeenth and eighteenth centuries, they are not at ease with Fourquin's French or with the Middle Ages (see particularly pp. 66-67, to be compared with pp. 60-61 of the original edition). Their footnotes, explanatory or otherwise, are embarrassingly inadequate and evasive (see especially pp. 56, 58, 164, 198). Worse still is their evident distance from the author himself. It seems clear (see particularly p. 193, n. 1) that the Sells had no opportunity to consult Fourquin in the course of their labors and that he had no occasion to review what they had done. In view of the disappointing quality of their work, one trusts that this was the case, and that, rather than supervising them, Fourquin was devoting his time and effort to research such as he has done in the past—careful, analytic studies firmly grounded in the medieval sources he edits so well, studies that have notably

advanced our understanding of late medieval France.

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C. T. ALLMAND, editor. *War, Literature, and Politics in the Late Middle Ages*. New York: Barnes and Noble. 1976. Pp. xii, 202. \$26.50.

This collection of specialized studies neither forms a unified whole nor gives a broad interpretation of the subject matter. Its value lies in bringing together the current research of a vital group of English and French scholars in an exciting new area of medieval studies. To call it a new area may seem strange, yet it is true that hitherto practical studies of warfare on the one hand and literary and legal studies on the other have not been brought together. This is now happening, and we are the richer for it.

One group of essays is concerned with the laws of war and chivalry. Hence it is fitting that the book opens with a tribute to G. W. Coopland, a pioneer scholar in this area. N. A. R. Wright explores a possibly important distinction between "law of arms" and "laws of war"; M. H. Keen skillfully advances our concept of chivalry in practice; C. A. J. Armstrong traces an unsavory case through the records of the *Parlement* of Paris, an admirable example of tedious research yielding a morsel of new knowledge. In a more strictly literary exercise, P. Contamine analyses three late treatises on war, correcting what we know of the life of one of the authors, Robert de Balsac.

In other essays, M. G. A. Vale adds a little to our knowledge of fifteenth-century artillery; J. R. Alban and C. T. Allmand break new ground in a study of fourteenth-century spies and spying. Two papers on Brittany yield new material on "bastard" or "decayed" feudalism (P. S. Lewis) and on the late medieval idea of a Breton nation (Michael Jones); while J. J. N. Palmer and A. P. Wells throw valuable light on the triangle between pope, France, and England during the pontificate of Urban V.

In the main, then, these essays might grace the pages of any learned review. The level of accurate scholarship is high; generalizations are minimal; our knowledge of the importance of war in the late medieval society is significantly advanced. These scholars are slowly, often painfully, bringing light into a dark area. Perhaps we can salute a new school of historiography.

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GORDON LEFF. *William of Ockham: The Metamorphosis of Scholastic Discourse*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xxiv, 666. \$47.50.

William of Ockham has long been the most challenging medieval philosopher for European historians. His positions were understood and used more than those of other philosophers to indicate close connections between philosophical and historical change. Recently, Lagarde, Moody, Boehner, Baudry, Maier, and others have tried to be precise about what Ockham actually said and thought. Gordon Leff's own important studies of fourteenth-century thinkers would have forced him "sooner or later to Ockham." Here it is.

Leff's methodology is to impose on Ockham's works a philosophical structure consisting of a complete list of his argued positions in about the order of writing. He keeps the sharp division of 1327 A.D. between the philosophical/theological and the political works. The relationship between these "must remain to some extent a matter of individual interpretation." The handling of this division is of great importance to historians, and we cannot be helped by a thematic integration with a rigid separation of reference. Leff makes no use of the political works before the short closing section on "Society," and does not use the pre-1327 works in it. Leff's three-part division—"The cognitive order," "The theological order," and "The created order"—works well as an analysis of this first group of writings. There are indexes of names, writings, and subjects, and a bibliography limited to Leff's sources and supplemental to the listed Ockham bibliographies. The subject index has flaws; its cross references do not always appear and some important terms are not adequately indexed. There is no index of Ockham passages cited.

In parts 1 and 2 the reader finds the subtitle justified. These parts are clear, difficult, and original. Ockham's radicalism is found in his theology rather than in his logic. Ockham, here, does not escape Aristotle, and this reader finds Leff's analysis of the *Logic* excellent, revealing the work to be more radical than Leff himself judges it. Leff has distinguished Ockham from his predecessors in cognition, concepts and universals, active and passive intellect, linguistic analysis, and demonstration. There are valuable evaluations of Ockham's positions both within his own system and in the philosophical train. In this he uses Lagarde, Moody, and Baudry, and he often demonstrates that Boehner's characterizations were wrong.

Part 3 is good reading, well-ordered and very different in theme from Arthur McGrade's *The Political Thought of William of Ockham* (1974). "Na-

ture" is a convenient discussion of scientific notions, often simply paraphrased translations taken seriatim from Baudry's texts in *Lexique*. The result is to separate Ockham from the long-sought medieval tradition in experimental science where Crombie put him. "Society" completes the book more than it examines the subject. Essays in this area (Miethke, Leff) have not yet made necessary this fashionable rubric change from politics to society.

Historians who want to update their appraisal of Ockham can still make do with Moody's 1967 article. McGrade's work, easier and clearer than Leff and too recent for his use, is good for the political and social thought. But Leff has elbowed his way into the company of Baudry and Lagarde, and historians who still think or hope or fear that history changes along with fundamental philosophical changes will have to use Leff, simply because his is the most detailed and responsible account so far. Baudry said that this task would not be easy. Leff shows that and more than that.

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S. F. C. MILSOM. *The Legal Framework of English Feudalism: The Maitland Lectures Given in 1972*. (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1976. Pp. xi, 201. \$18.50.

The publication of S. F. C. Milsom's Maitland lectures is a major event in legal history. He describes the feudal component in English society around 1200 on the basis of an analysis of the tenurial implications of the legal evidence in Glanvill and the early plea rolls of the reigns of Richard I and John. The common law concerning land came into existence during this period, in part, to enforce feudal custom. Milsom traces the development of property rights in the royal courts in the context of sanctions against a defaulting tenant, of claims to the tenement, and of grants and inheritance. He argues trenchantly from the evidence, with but a few glances at the more copiously documented world of Bracton and the *Quia Emptores*.

Milsom challenges Maitland's pervasive outline of the development of the forms of action. The sequence remains the same—first pleas in the right, then possessory assizes followed by writs of entry. But Milsom provides a quite different reason for the development of the new rights in land and the concomitant growth of royal jurisdiction. "Great things happened; but the only intention behind the writ of right, mort d'ancestor, and

novel disseisin was to make the seignorial structure work according to its own assumptions" (p. 186). The author sees the litigation of this period as a reflection of a vigorous feudal world such as Stenton has described. Yet it was a society destined to be changed because of the transformation of legal ideas as well as the transfer of jurisdictions.

The sources are difficult. There are no records from the lords' courts at the time when they were flourishing, while the multitude of cases from the royal courts often seem to be inscrutable. In an admirable demonstration of how to interpret plea rolls, Milsom draws much inferential evidence about the legal operation of the seignorial courts and shows that it is frequently possible to get at the realities behind formal statements in court records. Of necessity much of this work is learned conjecture with which not everyone will agree. His modest desire is to convince us that the world he describes cannot have been quite as we had supposed, and in this he more than succeeds. Unlike many publications of lectures, these are copiously annotated. The clarity of expression and the importance of the theme recommend the book to serious students as well as to specialists, to feudal historians as well as to legal scholars. It is fitting that the Maitland lectures serve as the forum for revising Maitland in the light of research inspired by his example.

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PRISCILLA HEATH BARNUM, editor. *Dives and Pauper*. Volume 1, part 1. (Early English Text Society, number 275.) New York: Oxford University Press, for the Early English Text Society. 1976. Pp. xvii, 359. \$12.50.

Students of late medieval England will applaud the appearance of the first volume of Priscilla H. Barnum's admirable edition of *Dives and Pauper*. A lengthy religious treatise written in vernacular prose by an unknown author between 1405 and 1410, *Dives and Pauper* enjoyed great popularity throughout the fifteenth century but lost its audience during the Reformation when religious interests changed. It suffered scholarly neglect until recently, largely because of the lack of a modern edition. This need has been met by Barnum, who, working from eight extant manuscripts and several fragments, has made available to scholars an important source of medieval religious and social history.

Ostensibly a commentary on the Ten Commandments, *Dives and Pauper* was addressed to the affluent and literate layman desirous of salvation, the Dives of the title. It filled a need unanswered

by late-fourteenth-century mystical works whose ideals of poverty and contemplation were not easily achieved by the worldly man of business. The Pauper, most likely representing the anonymous author, probably a preaching mendicant, brought his underlying mystical assumptions to the treatise but invested its contents with an eminently practical approach. The work begins with a prologue in which Pauper extols the virtues of Holy Poverty but quickly assures Dives that even a rich man may hope to enter Paradise if he dispenses his wealth wisely and lives judiciously. Pauper then leads Dives gently down the path to spiritual perfection through an explication of the Ten Commandments, although, in actuality, Pauper employs the Mosaic Law mainly as a device for elaborating upon the complexities of medieval Christian faith and the world it colored. Herein lies the value of *Dives and Pauper* for the historian: it is a treasury of insights into late medieval English life. Little escaped Pauper's notice and comment; among the many contemporary concerns observed are astrology, witchcraft, abuses of the clergy, alchemy, miracles, laxity of the laity, civil disobedience, comets, usury, vernacular Scriptures, Robin Hood, and warfare.

The present volume contains the text and variants of the Tables, Prologue, and Commandments I–IV. Volume 2 will cover Commandments V–X and their variants. Volume 3 will contain a lengthy introduction, with explanatory notes and a glossary.

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JEAN DEVISSE. *Hincmar: Archevêque de Reims, 845–882*. In three volumes. (Travaux d'histoire ethico-politique, number 29.) Geneva: Librairie Droz. 1975. Pp. 1561.

How much can be said about a ninth-century prelate? No reader should hazard a guess until he has mastered this massive two-volume, 1137-page text, which is fortified by 6,169 footnotes and a 345-page third volume of appendices and bibliography. What can be said in such abundance, even about Hincmar of Reims? No reader should expect a simple answer in this brief review; he must seek a response by a careful reading of this long "*itinéraire de décryptage d'une pensée, d'une vie, d'une époque*" (p. 1121).

Jean Devisse succeeds best in decoding Hincmar's thought; the book is an intellectual biography. It is organized so as to highlight in rough chronological succession certain crucial episodes in Hincmar's career. The issues raised by these events evoke subsidiary concerns which invite the author to treat significant topical issues such as

marriage, the social order, the nature of the church, royal authority, etc. Devisse's overriding concern is to describe and analyze Hincmar's intellectual reaction to the realities he confronted as archbishop, royal adviser, pastor, and Christian man. In this respect, Devisse's work is impressive. His awesome mastery of Hincmar's writings permits him to outline the substance of Hincmar's thought on any issue fully, clearly, and objectively. His demonstration of the sources of Hincmar's ideas and of the way the prelate exploited them to develop his own position, often a unique and original one, is a truly remarkable piece of scholarship. Devisse succeeds remarkably well, too, in drawing forth the consistent and persistent elements that emerge from Hincmar's dialogue with his times to form an integrated world-view. There is not space here to describe the rich substance of that view. Suffice it to say that Hincmar is portrayed as an assiduous miner of the traditions imbedded in Scripture, the Church fathers, ecclesiastical legislation, and even Roman literature in his search for "*le levier d'une action, . . . un outil de prédication*" that would permit him, as a man of action and a pastor, to instruct his contemporaries in what constituted right order in society. He is delineated as a Latinized "*barbare*" who struggled to use his Christian-Frankish cultural heritage as an instrument to compel his "flock" (usually defined rather broadly) to individual and collective godliness. Devisse's Hincmar emerges as the most mature product of the Carolingian renaissance; the study is well worth pursuing to discover not only why Hincmar deserves this distinction, but also what that entire effort amounted to.

The study is less successful in other respects. As an account of an important man's life, it tends to be diffuse and confusing. While the author touches upon every episode left in the record about Hincmar, his preoccupation with the prelate's written dialogue with his age tends to blur out the orderly progression of Hincmar's career and to distort the relative importance of successive events. A one-sided figure emerges from the study, a man invariably cerebrating as his prime reaction to a succession of crucial events over a long life. One suspects there were other dimensions to this intriguing figure; if so, they do not emerge very clearly in this study.

Devisse's study is least successful in deciphering "*une époque*." It sheds disappointingly little light on the broad forces generating tension and change in Hincmar's time. Repeatedly, Devisse's comments on the larger course of history between 845 and 882 center on demonstrating that kings, popes, bishops, nobles, priests, and ordinary Christians did not follow Hincmar's pastoral guidance, with the result that disorder, violence, avarice, and oppres-

sion increasingly disturbed the social order. His treatment of the political, social, and religious issues of the era almost persuades one that the cause of the dissolution of the Carolingian order was what Hincmar said it was: unrepented and uncorrected sin. Again, this is not especially illuminating.

Yet, one must not ask too much from one study. Devisse has done a remarkable job in illuminating the ideas that emerged from a serious, dedicated man's encounter with reality. He has done much to correct the innumerable distortions and misrepresentations that previous scholars have imposed on Hincmar. He has exemplified with remarkable skill a method of approaching Carolingian intellectual history. His careful respect for the Hincmarian texts and his assiduous search for the sources of the prelate's ideas are models well worth imitating. This is an important contribution to Carolingian scholarship.

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BRIAN PATRICK MCGUIRE. *Conflict and Continuity at Øm Abbey: A Cistercian Experience in Medieval Denmark*. (Opuscula Graecolatina, volume 8.) Copenhagen: Museum Tusculanum. 1976. Pp. 151. kr. 35.

The Danish Cistercian monastery of Øm was established in the twelfth century and suppressed in the sixteenth. As in the case of hundreds of medieval monasteries, neither its founding nor its four century existence nor its disappearance had more than regional significance. Yet the history of lesser monasteries is instructive in itself and can illuminate major topics of monastic history in new ways. This monograph is a respectable contribution to Cistercian and Danish history.

The development of Øm Abbey is not abundantly documented. Aside from brief references, the chief source is a volume composed at Øm between 1207 and 1267, consisting of four parts: a foundation narrative; an abbot list from 1165 to 1246 with brief characterizations of each; an account of Øm's struggle with Bishop Tyge of Århus over its exemptions; and a biography of Gunnar, Abbot of Øm and Bishop of Viborg. Any useful history of Øm must deal with this crucial source, which reflects the biases of monks who could not understand the views of their opponents. Brian P. McGuire was successful: critical without skepticism; sympathetic without gullibility; capable of exploiting the source without being trapped by the monks' perception of themselves, their friends, and their enemies. His account of Øm's first century is fairly balanced, readable, and even compelling.

The monograph's chief problem concerns its last three chapters which trace the monastery's history

after the Øm book ended in 1267. Sources are meager for the final three centuries, and the rich detail of the earlier sections gives way to surmise and summary. For instance, a 1554 library inventory is used to argue—unconvincingly—for a “cultural revival.” In view of the scanty sources, it is regrettable that the author was unable to use archaeological data effectively. The book contains three maps and seven photographs of the partially excavated site, but only two pages of text were devoted to the physical layout and building program at Øm.

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PAOLO CAMMAROSANO. *La famiglia dei Berardenghi: Contributo alla storia della società senese nei secoli XI–XIII*. (Biblioteca degli Studi Medievali, number 6.) Spoleto: Centro Italiano di Studi sull'Alto Medioevo. 1974. Pp. 416.

The Cartulario della Berardenga, a transcription made in 1230 of over 600 original charters of the monastery of San Salvatore di Fontebona outside Siena and edited half a century ago by Eugenio Casanova, forms the documentary base for Paolo Cammarosano's important study of the monastery's founding family. Established as a convent in 879 by Count Winigis of Siena, San Salvatore was refounded as a monastery in 1003 by his descendants. Over the centuries, the family's relationship with its monastery changed. Direct involvement in the convent, whose abbesses were chosen by the family from within its ranks, was replaced by a looser lordship over the eleventh-century foundation, whose abbots, although still appointed by the Berardenghi, were no longer necessarily their kinsmen. In the twelfth century, Berardenghi lordship declined into patronage. The community at San Salvatore formally joined a monastic order, the founding family now merely gave consent to the abbot's investiture, and earlier pious donations to the monastery began to be replaced by sales.

Yet its joint monastic commitment may have served to bind the family together. The sole act that united all of its male members was their ratification of the monastery's new twelfth-century constitution. The charters generally characterize a family whose strongly patrilineal orientation had always encouraged it to slough off collateral branches, promoting the multiplication of individual, self-sufficient lines. The economic unity of the Berardenghi seldom extended to cousins, and by the first quarter of the twelfth century even a traditional solidarity among brothers seems to have declined. With economic independence came the

political and social isolation of individual familial lines, which exerted control over particular territory from their separate castles.

What then of Jacques Heers' urban "clan familial," which he has described as a continuation within the medieval city of dense familial structures of the aristocratic countryside? Only exceptionally was a Berardenghi castle in the countryside held by a *consorteria* of distantly related family members. Yet the group of Berardenghi brothers who immigrated to Siena, forming a compact settlement around an urban church in the control of San Salvatore, passed on to their descendants, who took the name Ugurgieri, a cohesive family structure in which a crowd of brothers, uncles, and cousins shared a common geographic, economic, and social existence.

Although unnecessarily wordy and ponderous, this study is an important piece of a complex puzzle of medieval familial structures and relationships. It concludes with a helpful appendix on money and prices.

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GIROLAMO GANUCCI CANCELLIERI. *Pistoia nel XIII secolo: Saggio storico sulla stirpe dei Cancellieri di Pistoia*. Florence: Leo S. Olschki. 1975. Pp. 429.

The Cancellieri family of Pistoia played a prominent role in the history of the city from approximately 1200. Its chief claim to renown beyond the annals of local history is the feud between so-called Blacks and Whites which broke out in its ranks in the late thirteenth century. The feud took on the dimensions of a civil war, spread to engulf Florence, enlisted the participation of Dante Alighieri, and led to the poet's permanent exile from his native city. The Cancellieri lineage became extinct in the male line in the eighteenth century, but a surviving daughter, Maddalena, married into the Ganucci family of Florence. The descendants of this union adopted the name Ganucci Cancellieri; one of the descendants, Girolamo, is the author of this present work.

The author is not a professional historian, and the text is largely a celebration of the *casata* of the Cancellieri and the role it played in the history of Pistoia in the thirteenth century. Ganucci Cancellieri relies heavily on standard authorities for his basic information—Zdekauer, L. Chiappelli, Santoli, and others. On many topics he simply prints verbatim passages drawn from contemporary chroniclers and recent historians, as if they were of equal value. The text, rambling in places to the point of incoherence, is sorely lacking in originality. Perhaps the book has value as a kind of

compendium of thirteenth-century and later citations alluding to the Cancellieri. But this small service seems hardly to justify the expense of publication.

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IRMA VALETTI BONINI. *Le Comunità di valle in epoca signorile: L'evoluzione della Comunità di Valcamonica durante la dominazione viscontea (secc. XIV–XV)*. (Scienze storiche, number 13.) Milan: Vita e Pensiero, Pubblicazioni della Università Cattolica del Sacro Cuore. 1976. Pp. vi, 293.

This work studies the development of a formal identity and sense of community in a modestly productive Alpine valley, answerable to no "*dominus loci*," yet lacking even the physical requisites for unity. Irma Valetti Bonini turns structural history inside out, seeking the underlying realities of Valcamonica by deliberately focusing on a labyrinthine interplay of political forces from the thirteenth to the fifteenth centuries. Bonini accepts economic explanations neither for Visconti designs nor for local evolution. Rather, she describes an intricate process that began with Visconti mediation of Brescian tussles and ended only with the reaffirmation of local family solidarities in a new guise. During the fourteenth century, successive Visconti rulers, Brescian factions, and main valley families juggled feudal convention and administrative reform that simultaneously exploited and denied Brescian control over a remote Ghibelline hinterland where imperial eagles hovered to seize papal keys.

This skillful, sometimes inspired commentary on two principal sets of unpublished material exposes inadvertently, as it were, the subtleties of colonial status and thence of the very economic exploitation the author is at pains to minimize. Induced eventually to take a cosmopolitan view of valley interests, the separate Valcamonican communities drew together. Their individual structures, however, remain shadowy in this work, not just because direct evidence about local magistracies and their bases of power is so elusive, but because of actual tensions between any spurious unity and independence proper. The Venetians, who ultimately absorbed the valley once the hapazard hegemony of the Visconti waned, clearly did not overlook, as Bonini has, the economic import of communication networks.

Forewarned by G. Procacci about the creative resilience in Italy of recycled feudal forms during 1,500 years of transition to the modern world, historians are, nonetheless, likely to find Bonini's approach intriguing, informative, and methodologically profitable. A valuable analytical index of

the episcopal registers of Brescia and the archives of the Federici family stabilizes the book's consciously chosen framework. Potentially, the data illuminate the economic transition implied in Bonini's account of a tenurial crisis separating the destinies of greater and lesser nobility.

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Macek, R. Nový, and J. Mezník remains untranslated. It is their work to which Western scholars need access if they want to understand better the place of the Hussite revolution in European history and to develop insights into the relationship between the material conditions of people's lives and their religious and political ideals.

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ROBERT KALIVODA. *Revolution und Ideologie: Der Hussitismus*. Translated by HEIDE THORWART and MONIKA GLETTNER. Cologne: Böhlau Verlag. 1976. Pp. xii, 397. DM 62.

This volume is a revision and translation of the author's *Husitská ideologie* (1961). He describes the ideological structure of the Hussite movement as one of the main forces attacking the late medieval social and political systems. Hussitism was the culmination of a series of European popular movements that reflected the crisis resulting from the commercialization of the late medieval economy. Robert Kalivoda sees the Hussite revolution as Europe's first bourgeois revolution. The book is a philosophical analysis of the ideas of the various trends within the Hussite movement: of Wyclif and Hus, of pre-Hussite heresies such as the Waldensians, Cathars, and Free Spirits, and of radical Hussites including the Chiliasts and Taborites. The author's strongest contribution is his section on Hus.

Hus taught his followers that a lord has rights to authority and dominion only if his actions further the interests of all classes in society. Dependents need only obey if the lord fosters the general welfare. In line with Wyclif's philosophy of realism, one's lord is not a ruler because the idea of "ruler" has been projected onto him from God; subjects should rather rely on their senses and obey a lord when they recognize him as such by his actions. Wyclif and Hus thus implied the concept of popular control over government.

Although clearly presented, the third (Taborite thought) and fourth (Hussitism and bourgeois revolution) sections are less useful. Except for Kalivoda's Marxist formulations, his treatment of Tabor provides little of substance which cannot be found more readily in Howard Kaminsky's studies.

One can only welcome the translation of the works of Czech Marxist scholars into Western languages. I doubt, however, if Kalivoda's work on ideology is the kind which is most helpful in understanding the nature of the Hussite revolution. The primary focus of the work on Hussitism available in Western languages has been the ideas of the movement, but a great deal of economic history produced by people like B. Mendl, F. Graus, J.

MODERN EUROPE

JAN DE VRIES. *Economy of Europe in an Age of Crisis, 1600-1750*. New York: Cambridge University Press. 1976. Pp. xi, 284. Cloth \$14.95, paper \$4.95.

Despite its usefulness to both scholars and students, the historical handbook has not become an adequately exploited format in the United States. This is in fact, if not in self-description, such a handbook; we are now provided with not only a skillful interpretation of a wide range of monographic literature relating to the dynamic elements of economic development in early modern Europe, but also a clear analysis of data which takes advantage of the concepts of economic theory. The area dealt with does not actually coincide with Europe, but in concentrating on England, western and west central Europe, and the western Mediterranean, Jan de Vries nonetheless surveys a larger region than is commonly attempted.

The period itself has a particular unity in economic history because it has been characterized as an era of stagnation or of no more than modest growth between two periods of secular economic expansion. That characterization is suited in particular to demographic trends for the continent as a whole and to the nullification of growth in north-western Europe by contraction elsewhere, in particular in the Mediterranean basin. The cause of the change in secular trends between about 1570 and 1650 is related here to factors within the European economies and their social structures, a procedure which emphasizes de Vries' attention to economic development and the resultant neglect of climatic, biological, and ecological trends as causative forces. In organization this study follows a scheme which is by now traditional, beginning with population trends and proceeding via agriculture, industry, and trade to the more amorphous sectors of capital, capitalism, and government intervention in the economy. Submerged within this framework, however, is a perceptive analysis of gains in productivity (and in particular of those associated with the use of less costly methods of production), of changes leading toward labor spe-

cialization, of movements in factor availability and consumer demand, and of market development in response to population redistribution. Strangely enough, de Vries characterizes the period as a whole as an age of crisis. It is true that the label "depression" does not apply satisfactorily, and also that the stagnation of the European economy as a whole, resulting from approximate stability in population size and the balance of regional variations in economic trends, has the effect of obscuring important areas of dynamic growth. No one will dispute the notion that this period encompassed economic, social, and political developments which should be interpreted in light of the concept of crisis—instability marked by impending decisive change. But it stretches the meaning and usefulness of the word "crisis" to employ it to describe the entire period and thereby to exaggerate both the level of instability and the eminent nature of decisive change.

Among several particularly fruitful suggestions for consideration and for further research, de Vries offers an analysis of agricultural and industrial change by means of a price-induced reallocation of labor model. The suggestion is that population stabilization during the seventeenth century, in dampening demand for basic foodstuffs, assisted the development of a rural proletariat engaged in manufacturing under the putting-out system of industry, a development which led toward labor specialization and which also stimulated market-oriented consumption in the areas affected, most notably in England, the Dutch Republic, Switzerland, the Spanish Netherlands, and parts of France and Germany. Not only is this a skillful conceptualization of growth forces in what appears at first glance to be an era of unrelenting depression, but also it coincides with and reinforces de Vries' emphasis on the importance of regional commercial exchanges in the development of northwestern Europe. Such ideas which prompt reflection are too numerous in this book to permit one to accept the author's modest statement of intended readership, which consigns the book to an audience composed only of undergraduate and graduate students.

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WILLIAM JAMES ROOSEN. *The Age of Louis XIV: The Rise of Modern Diplomacy*. Cambridge, Mass.: Schenkman. 1976. Pp. 208. Cloth \$11.25, paper \$5.95.

This is an able presentation of diplomatic practices and institutions in Europe from 1648 to 1715. Though the work of theoreticians is cited, the au-

thor concentrates on the actual conduct of diplomats and their masters. The majority of examples are drawn from English, French, Swedish, and Dutch diplomacy, with abundant material from other states as well. After summarizing the high politics and wars of the period, William James Roosen treats the different kinds of central administrations across Europe and the ways in which foreign policy was formulated. Ensuing chapters deal with ambassadors: envoys, residents, consuls, and diplomatic secretaries; the gathering and transmission of information, including courier and postal service, ciphers, spying, and deciphering enemy mail; and a description of the general round of diplomatic duties from negotiations and court ceremonies to the protection of countrymen abroad.

The author demonstrates how much can be done with material that is already in print: documents, letters, diaries, memoirs, and travel descriptions, as well as quotations in books and articles. These are supplemented by his own archival research in Paris and London and by manuscript material from the Huntington Library. Chapter 5 ("A Typical Early Modern Embassy") exhibits Roosen's resourcefulness in weaving together disparate material into a convincing description of an ambassador's activity, including the settlement of personal affairs.

In discussing the near absence of career training for future ambassadors, Roosen might have made an exception for nuncios, who were selected early in their careers, seasoned by political and administrative responsibilities in the papal states, and then given ever more challenging assignments in successive postings abroad.

Contrasts and comparisons between seventeenth-century practices and those in today's world are generally well done. A propos the substance and symbolism of protocol disputes, comparisons might have been made with recent controversies such as that over the shape of the table(s) at the Paris negotiations to settle the Vietnam War. Moreover, the subtitle of the book, "The Rise of Modern Diplomacy," is puzzling. Roosen himself states that "in terms of practice and institutions, diplomacy was carried on in essentially the same way in 1715 as it had been in 1648" (p. 189).

The select bibliography is well chosen; an appendix includes sample letters of credence and recall, passports, and ciphers. Unfortunately, Roosen has been ill served by his publisher. In at least thirty-five instances, notes which refer to other sections of the book do not cite specific page numbers. Thus we find notes reading, "See above, Chap. III, pp.—" This would be a mere blemish if an index were provided. Such lapses, however, should not be attributed to the author of this well

organized study that will render considerable service to students of the early modern period.

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THERESA M. MCBRIDE. *The Domestic Revolution: The Modernisation of Household Service in England and France, 1820-1920*. New York: Holmes and Meier. 1976. Pp. 160. \$18.00.

This book treats an often neglected but important subject: the occupation of the domestic servant during the nineteenth century. In both England and France large numbers of young women migrated to cities to enter "service." Even though the situation of female textile operatives was discussed more widely, many more women were domestic servants in the age of industrialization. Yet few books before this one have systematically analyzed their history.

Theresa McBride has collected a good deal of information about her subject. She has traced the geographic origins of servants in several French cities and found, not surprisingly, that servants were recruited from rural areas primarily. She has gathered interesting evidence on their social origins and on marriage patterns which leads her to conclude that, through their marriages, servants often experienced a degree of upward social mobility. Educated by their mistresses or simply by exposure to middle-class life, some servants went on to higher status jobs. Others accumulated substantial savings. The book also contains data on wages and on the economic benefits of servanthood. McBride contrasts servants' wages with those of women in skilled and semi-skilled jobs. From her analysis she concludes that "domestic service offered a distinct monetary advantage over other kinds of female employment in the nineteenth century."

The book details the economic and social advantages of servanthood and also its miseries. It documents cases of downward as well as upward mobility and sensitively examines abuses and hardships a young girl might face. Servants might be fired arbitrarily by a demanding mistress. More often than other female occupational groups they were the mothers of illegitimate children. The instability of their employment sometimes forced them into prostitution.

Using advice manuals to housewives, and newspaper and magazine accounts, the author depicts the kinds of work servants did and the varied relationships they had with their employers. McBride uses many different types of sources to create a detailed and nuanced study of the occupation of the servant in the nineteenth century.

The conceptual framework, however, is vague and adds little to the presentation. The book is

supposed to be about the "modernisation of household service." McBride says that "domestic service has played a pivotal role in the modernisation of women" and that "modernisation came largely as a result of the broadening of roles for women outside of . . . traditional familial ones." Yet she tells us elsewhere that domestic service was a "traditional" occupation for young girls. Well before the nineteenth century, becoming a servant meant migrating. It also meant leaving one's family. What then, aside from an increase in the size of the occupational group, changed during the nineteenth century? The answer is difficult to find in this book. And what does "modernisation" mean? The term is not defined though it is used repeatedly as if its meaning were self-evident. The impact of the job on servants is clear: it helped them migrate and adjust to cities. It offered an alternative to impoverished rural life. But was there a larger effect? McBride suggests that there was a "domestic revolution," related somehow to modernization, but the nature of the revolution is not spelled out.

I think the book would have been better without "modernisation." The concept is largely irrelevant to the material presented and it is not needed to enhance the value or the interest of the subject. Indeed, McBride's book proves that the occupation of the domestic servant itself is an important and interesting topic worthy of the kind of historical treatment she gives it.

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GERALD N. IZENBERG. *The Existentialist Critique of Freud: The Crisis of Autonomy*. Princeton: Princeton University Press. 1976. Pp. xii, 354. \$16.50.

This book provides us with a needed intellectual history of existential forms of psychoanalysis. Gerald N. Izenberg sets the theories of Freud, Heidegger, Binswanger, Boss, and Sartre in the context of the crisis of rationalism. The book begins with an explication of Freud's theory, treating it as a form of biologism. The following chapters treat the existentialists' effort to go beyond the apparent inadequacies of Freudianism by comprehending neurosis through the categories of inauthenticity and bad faith. The existentialists tried to develop a psychological theory that would transcend the concepts of sexual determinism, mechanistic causality, and the unconscious by disclosing how consciousness fled from its own freedom. Izenberg concludes that none of the efforts were totally successful, although they did confront the problems of modernism better than traditional rationalism and psychoanalysis.

The author has organized his essay well and

writes on these difficult matters with splendid clarity. He is most convincing in his treatments of Heidegger, Binswanger, and Boss. His discussion of Freud is marred seriously by omitting the work of Jacques Lacan, a French analyst who disputes the mechanist, biologicistic interpretation of Freud which the author adopts. Lacan finds in Freud just that theory of meanings and symbols which Izenberg says is lacking.

The weakest part of the book is the section on Sartre's *Critique of Dialectical Reason*, which Izenberg judges to be a "very crude biology and sociology." Thus Izenberg fails to see the importance of Sartre's concept of mediation, misleading the reader into thinking that Sartre reduces social life to biological needs. Next, the author incorrectly views the *Critique* as a radical break with Sartre's earlier concept of freedom by omitting the ontological problem—hardly a plausible claim to anyone who has read Sartre. Izenberg even asserts, again falsely, that Sartre does not take up the question of the psychology of the ruling classes in the *Critique*, whereas in fact he devotes a long section to the topic in terms of the problem for the bourgeoisie of distinctiveness. There is also a brilliant section on colonial ruling-class psychology in the *Critique*, which Izenberg apparently failed to notice. Hence he concludes that Sartre's existential psychoanalysis fails for lack of an adequate social theory. But one could more properly argue the opposing position that the *Critique* and the study of Flaubert (which Izenberg omits) provide just those social categories needed for a full understanding of consciousness.

Aside from these lapses, Izenberg has given us a fair, well-researched, intelligent, and readable account of an important chapter in twentieth-century intellectual history.

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EUGENIA V. NOMIKOS and ROBERT C. NORTH. *International Crisis: The Outbreak of World War I*. Montreal: McGill-Queen's University Press. 1976. Pp. xv, 339. \$23.00.

In their introduction the authors declare they are not attempting to improve on standard works or enter the old war guilt controversy. Instead, they are interested in "identifying the processes of crises" with the object of developing a theory of international behavior from which they might derive lessons relevant to past or future crises. Obviously July 1914 is a classic example of the escalation of a local problem into a world war, and Eugenia V. Nomikos and Robert C. North try to demonstrate in over fifty short chapters how the

unfolding situation appeared in each of the major capitals. The historical account is based on a reworking of published documentary collections, heavily supplemented by Albertini. The final two, largely theoretical, chapters display the whole—one must say ghastly—apparatus of the behavioral science approach with dense jargon to belabor the obvious in the type of language which refers to a diplomatic history of 1870–1914 as "an analysis of the buildup of conflictual interchanges among major powers." However, the ten lessons the authors finally present are harmless enough and full of common sense. Examples are: "A good turn (conciliatory response) on the part of one's opponent should be rewarded (reciprocated)," and "avoid inflaming your own public opinion."

But even as a study of the "processes of crisis" the book is disappointing. This is largely because the analysis relies almost exclusively on *published* documents to deduce the perceptions of various statesmen. Anyone who has worked in the archives knows that these collections represent only the tip of the iceberg. Therefore when Nomikos and North remark, for example, that the 1914 crisis documents contain remarkably few references to capability compared to the prewar naval and military debates (p. 260) they should realize that the editors of the publications may have omitted for reasons of space material on this very subject. One never really knows until one digs in the archives, and the latter have been, for the most part, open for over a decade. Consequently a number of valuable monographs have been published, and the authors' failure to exploit this literature is a serious mistake. Little of what they did use was not available almost two generations ago. How can one really explain the British position without reference to Samuel Williamson's exhaustive work on the Anglo-French staff talks, or why use just *The World Crisis* when we now have Randolph Churchill's biography of his father with its massive companion volumes? There are similar omissions, like Jarausch and Dedijer, for the other countries. Even in the case of published documents the authors might have performed a genuine service by incorporating Italian material which was not available to Albertini. The relevant volume of *I documenti diplomatici italiani* appeared in 1964, and there is no excuse for not using it. Nomikos and North had an interesting idea but they used an excessively narrow range of materials to carry it out.

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ERWIN HÖZLE. *Die Selbstentmachtung Europas: Das Experiment des Friedens vor und im Ersten Weltkrieg*. Göttingen: Musterschmidt. 1975. Pp. 601.

This study, by one of the doyens of German diplomatic historians, traces the dynamics of European international relations from roughly 1890 until 1916, i.e. until the end of the limited coalition phase of World War I. The analytical and interpretive framework of the book is derived largely from traditional diplomatic history. Within that framework, Erwin Hölzle orders a variety of causal relationships of social, economic, political, ideological, and psychological nature. He exploits an extraordinarily wide range of old and new sources including documents, memoirs, diaries, biographies, periodical literature, and monographs. Hölzle's thesis is that the independence of Europe and its world power were conditioned on the preservation of peace on the European continent. The outbreak of World War I and the stalemate of the war dissipated European power, revolutionized its society, shook its superiority over the colonial world, and destroyed its independence of outside powers.

According to Hölzle, the "experiment of peace" failed and the July crisis arose and became transformed into war because the great powers in their pursuit of *Weltpolitik* and egoistic national interests became blind to the requirements of maintaining the balance of power. Hölzle's interpretation of this balance as an actual or potential peace system is, in my opinion, misconceived. Peace was not the primary goal of the balance of power system before 1914; in fact peace was only incidental to it. The system assumed the aggressive and competitive character of relations between states and was dominated by great powers striving to expand as well as preserve their power. War was the logical and legitimate means of adjusting the relations between the powers generated by continuous competition for power; one could argue that 1914 represented not the failure of the balance of power but its normal operation.

Thematically, Hölzle's book is a reply to Fritz Fischer's thesis that in its pursuit of the status of a *Weltmacht*, Germany deliberately faced the risk of war with Russia and France and had to bear a substantial share of the historical responsibility for the outbreak of the war. Hölzle, one of Fischer's most persistent critics, is at pains to salvage as much as possible of the pre-Fischer version of Berlin's strictly defensive role in the outbreak of the Great War and the genesis of its war aims in the wartime situation. He stresses what he views as the objective reality of the encirclement of Germany by a ring of enemies intent on crushing it. He attributes great importance—exaggerated in my opinion—to rumors in May 1914 of an Anglo-Russian naval convention and a planned landing in Pomerania (p. 379), and England emerges as the villain of the book. Out of concern for its world position and the security of India, England failed

to restrain Russia in Europe. Read closely, however, Hölzle's conclusion on Germany's role in the outbreak of the war really approximates Fischer's views. In July 1914, German Chancellor Theodor von Bethmann-Hollweg took a "calculated risk" that involved supporting a local Austro-Serbian war as a way of testing the cohesion of the Entente and Russian will to war (pp. 389–391). Subjectively, Bethmann-Hollweg's calculated risk may be described as defensive, but objectively it was offensive in the sense of political action. Bethmann-Hollweg may not have aimed at a great war, but from Hölzle's study it is clear that he was prepared to run the risk of triggering it.

Hölzle denies that Germany had any war aims that required war for their attainment; he contends that German plans for hegemony in Europe came to the fore only after the outbreak of the war (p. 380). He fails to explain how these plans were so quickly formulated after the outbreak of war—the notorious German war-aims program of September 9, 1914 discovered by Fritz Fischer—if they did not exist in serious political circles before the war. Germany, according to Hölzle, had no war aims even after the outbreak of the war; Germany only had "peace aims" and a "security policy" (pp. 435, 437). Leaving aside Hölzle's semantic distinctions, his account of Germany's "security needs" can again be seen as approximating Fischer's views. Exonerating Germany of all responsibility for the failure to restore peace, Hölzle places the blame for the refusal of France and tsarist Russia to conclude a separate peace with Germany on the "insurmountable wall" (p. 592) of the London Treaty of September 5, 1914 in which each of the Entente Powers pledged not to do just that. This blinds him to the normal considerations of great-power politics at the time. For France and Russia the preservation of the Entente, quite apart from the London Treaty, was necessary for their great-power aspirations. This was especially true of Russia; the more military success Germany had the more Russian survival as a great power depended on its allies.

Hölzle's assumptions and interpretations should be questioned. Nevertheless, his detailed scholarship will prove valuable to all historians seeking to construct an historical explanation of the outbreak of the Great War.

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THEDA SHAPIRO. *Painters and Politics: The European Avant-Garde and Society, 1900–1925*. New York: Elsevier. 1976. Pp. xx, 341. \$15.00.

Theda Shapiro has boldly tried to bridge the gap between art history and history by analyzing the

political and social attitudes of "a generation of painters who wrought a revolution in the arts"—the Futurists, Expressionists, Fauves, and Cubists (p. xii). Positing the existence of a self-conscious avant-garde of eighty painters who came to maturity just at the turn of the century, Shapiro traces its interaction with the political Left. She tackles both the methodological problems of art and history and the historical phenomenon of the identification of avant-garde art with radical politics.

For both of these reasons, this book is valuable for historians, particularly those who seek to understand the formation of public images and opinion. Shapiro's book illuminates the group mentality out of which revolutionary artistic images emerged and the confusion that early developed between what was artistically revolutionary and what was politically revolutionary. Implicit in this book is the conviction that drastic changes in artistic images have serious social repercussions which historians have neglected. The social and political sources of these changing images as well as the reaction to them needs to be seriously considered by historians.

Through an analysis of group life-styles, of individual diaries and letters, of manifestoes, and of artistic and political activity, she traces the attitudes of painters to prewar social conditions, to the war, and to postwar revolutions. The humanitarian anarchism of the painters which resulted in their support of and by the Left rested upon their commitment to art itself as a revolutionary force, a commitment which was in the final analysis not compatible with political revolutionary demands.

By using the concept of the avant-garde, the author is able to delineate characteristics which were common to the painters of different schools and countries and which are useful to understanding the relationship between painters and society. At the same time, one is left with an occasional sense of distortion which may be unavoidable in a group biography. More fundamental in the problem of approaching art as historical document is Shapiro's use of specific works of art and her analysis of "stylistic predilections" to demonstrate attitudes or ideology. Here the subjectivity of choice and interpretation is most marked, even though the attempt is laudable and her arguments are convincing. Can one, for example, interpret the rejection of materialistic representation as a profoundly political stance in prewar years (pp. 84-5) and a continuation of those trends after the war as a retreat from politics (p. 180)?

Shapiro has used both artistic and political archives and documents; she has interviewed artists and consulted police records; her bibliography is well balanced and extensive in French, German, and Italian sources. Finally, she has provided ex-

tremely useful biographical sketches of the avant-garde painters.

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DANIEL H. THOMAS and LYNN M. CASE, editors. *The New Guide to the Diplomatic Archives of Western Europe*. Philadelphia: University of Pennsylvania Press. 1975. Pp. xi, 441. \$10.00.

The principal changes in this second edition of the "scholar's *Baedeker*" are the new chapters on the archives of Finland, Greece, and Luxemburg and an extensively revised section devoted to the United Nations and other international organizations, including two new subsections on the International Labor Organization and the International Telecommunications Union. The chapter on public opinion and foreign affairs has been eliminated, but material on periodical collections has been included with the other information on the various states. Similarly, the chapter on Bavaria has been integrated into the greatly expanded section on Germany. The other chapters have updated bibliographies and current information on the operation of the archives and the general conditions awaiting the travelling scholar.

As in the earlier volume, the individual chapters have been prepared or revised by well-known scholars. Each chapter contains a brief history of the depositories and their collections, a description of their organization and the manner in which specific holdings are classified, a section on the administration and operation of the archives, and a bibliography of published documents, printed guides, and inventories.

Of all the improvements, I would single out the excellent new chapter on Germany by Fritz Epstein as being by itself worth the modest price of the book. There are a few gaps, such as the failure of the chapter on Austria to mention the massive microfilming of Austrian documents by the Library of Congress, but the *New Guide* is still the best place to begin when one wants to do research in the major Western European archives.

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DOREEN COLLINS. *The European Communities: The Social Policy of the First Phase*. Volume 1, *The European Coal and Steel Community, 1951-70*. London: Martin Robertson. 1976. Pp. viii, 128. \$11.50.

The first of two volumes concerning the social policy of the European communities, this brief study focuses on the European Coal and Steel Community (ECSC), 1951-1970. We may hope that the sequel will treat the European Economic

Community (Common Market), 1958–1972, in more detailed and analytical form. While it is difficult to assess one volume apart from the other, volume one is a useful and concise study.

Beginning with a discussion of the treaty which established the ECSC, Doreen Collins presents a topical treatment of the social impact of the coal crisis, which had assumed major proportions by 1957. Largely because of the slump in the coal industry, the ECSC had found it necessary by the late 1950s to define its role in such increasingly broad and complex areas as readaptation programs, retraining programs, and area redevelopment efforts, none of which had been specifically anticipated in the language of the treaty. Coordination with governments and with worker and employer associations, as the author indicates, has been the key to success in the articulation of the largely undefined social policy responsibilities of the ECSC. The development of social policies intended to facilitate “equalization” throughout the Community is identified as the greatest continuing challenge. Housing policies, the migrant worker, adult and managerial training, and health issues are discussed only briefly.

A further elaboration of specific problems facing the ECSC is needed. A discussion in greater depth of the role developed by the ECSC in the formulation and pursuit of social goals in the collaborative, sometimes directive context with employers, workers, and national governments would have resulted in a more useful study. It is, in fact, introductory and broadly descriptive. While the tables and charts are useful, the bibliography is not. Only four journal articles are cited, and all the sources are readily available. Neither does the author appear to have interviewed ECSC personnel directly involved with the development and implementation of social policy. Otherwise, the book is competently written and organized. Certainly the role of this regional organization in both the development and implementation of social policy in a context where important areas of decision-making reside elsewhere (i.e., national governments and worker and employer organizations) will need further, more detailed study.

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NICHOLAS ORME. *Education in the West of England, 1066–1548*. Exeter: University of Exeter Press. 1976. Pp. xiii, 239. £7.50.

This is a study of the one hundred twenty known schools in the six counties of Cornwall, Devon, Dorset, Gloucestershire, Somerset, and Wiltshire between the Norman Conquest and the Reforma-

tion. In a lengthy introduction Nicholas Orme sets the stage by discussing the general features of English school education in the period: types of schools, their distribution and continuity, endowments, masters and pupils, and the impact of the Reformation on education. In essence, this is a summary of Orme's standard, *English Schools in the Middle Ages* (1973), with special reference to the West of England.

The major portion of the book is given over to histories of individual schools. For comparative purposes, these are considered in four groupings: schools of the five principal cities of Bristol, Exeter, Gloucester, Salisbury, and Wells; fee-paying schools of the other towns and villages; endowed and chantry schools from the fourteenth century onward; and educational institutions of religious houses. The emphasis is upon institutional history, upon specifying the origins, continuity, constitution, and lives of benefactors, masters and pupils of individual schools. Seven maps assist the process, along with three genealogies, a fine bibliography and an index.

The sources permit relatively complete accounts of the schools of the principal cities and the endowed and chantry schools, but only thin accounts and brief references to the fee-paying schools of the towns and villages and the schools of religious houses. Sometimes Orme's diligent search of an extensive array of primary sources (deeds, wills, audit books, tax rolls, parish registers, etc.) has turned up no more than a tantalizing hint, as in the entry for Stow-on-the-Wold, Gloucestershire: “Among those who paid the poll tax at Stow in 1381 appears a John Scholmarster, a layman keeping a servant” (p. 102). Even so fleeting a reference may be tremendously useful, of course, and we may be grateful that he has given us all that he has found in an authoritative and readable book whose value for the study of both general and local history is enormous.

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R. W. HEINZE. *The Proclamations of the Tudor Kings*. New York: Cambridge University Press. 1976. Pp. xii, 317. \$24.50.

FREDERIC A. YOUNGS, JR. *The Proclamations of the Tudor Queens*. New York: Cambridge University Press. 1976. Pp. xiv, 277. \$24.50.

The use of royal proclamations in Tudor and early Stuart England has been a topic of interest since the period itself. Sir Edward Coke's opinion of 1610—“the King by his proclamation . . . cannot change any part of the common law, or statute law, or customs of the realm; . . . that which can-

not be punished without proclamation, cannot be punished with it"—is famous but so extreme as to be misleading, while Henry VIII's equally famous Statute of Proclamations (1539) has been subjected to a variety of contradictory and unsupported interpretations. There has thus been a long-felt need for a careful study of Tudor royal proclamations, analyzing their scope and discussing their enforcement, function in society, and constitutional significance.

This study has now been completed. Undertaken in two stages, it is the work of four historians—all, let it be noted, Americans. First Paul L. Hughes and James F. Larkin published an authoritative text of the documents themselves in the three volumes of *Tudor Royal Proclamations* (1964, 1969). (The earlier *Bibliography of Royal Proclamations* published by Robert Steele in 1910 provided only summaries.) Once the evidence was easily available analytical studies could be made. Hughes and Larkin did not undertake this themselves—they have now gone on to the editing of Stuart royal proclamations—but they encouraged the work of two young scholars: Rudolph W. Heinze, who wrote a dissertation on early Tudor proclamations while a student of Robert Kingdon at the University of Wisconsin, and Frederic A. Youngs, Jr., who migrated to Cambridge in order to study Elizabethan proclamations with G. R. Elton. In 1967 Heinze and Youngs met. Their co-operative enterprise has given us the present definitive study.

Both books begin with a definition of proclamations. As Youngs puts it, "a royal proclamation was a royal command, normally cast in a distinctive format, which was validated by the royal sign manual, issued under a special Chancery writ sealed with the Great Seal, and publicly proclaimed" (pp. 9–10). Applying these criteria, Heinze and Youngs exclude certain items printed by Hughes and Larkin on grounds that they were not royal commands, were not proclaimed, were purely local, or were royal orders issued in another form, such as letters patent, circular letters, or Church briefs. In all Heinze excludes 21 items, Youngs 119. Both authors add a few proclamations missed by Hughes and Larkin. The resulting canon consists of 839 items: 58 issued by Henry VII, 200 by Henry VIII, 115 by Edward VI, 64 by Mary, and 382 by Elizabeth.

Obviously the frequency with which proclamations were used varied considerably. So few proclamations survive for the period prior to 1529 that Heinze speculates that some have disappeared without trace (we have the topics, but not the texts, of others.) Under Thomas Cromwell, and especially after the passage of the Statute of Proclamations, the frequency rises, yet the climax came

only after the Statute's repeal during the Protectorate of the Duke of Somerset. Northumberland and Bloody Mary issued proclamations sparingly, but under Elizabeth their use was again frequent and their impact considerable.

Proclamations served various functions and dealt with a number of important topics. Except in Mary's reign, when domestic security was the most pressing issue, most proclamations dealt with economic matters, regulating such things as wages, prices, export and import, and standards of manufacture. Substantial numbers also concerned social regulation (for instance, the apparel appropriate to different classes), foreign affairs, administrative matters, and, especially under Mary and Elizabeth, religion. Both authors point out the constitutional role of proclamations as temporary legislation, designed to deal with emergency situations until Parliament could react with statutes; the early stages of both the Marian and Elizabethan religious settlements were thus effected by proclamation. Proclamations could also modify or suspend unworkable legislation, such as that dealing with the slaughter of cattle or the price of wine, order enforcement of neglected laws, demand the end of rebellion, serve as propaganda with regard to war and peace, or authorize the prosecution of religious dissenters, both Puritan and Catholic. There were closely related to parliamentary legislation and never touched the land law or common law generally.

Anyone who peruses Steele or Hughes and Larkin will have a rough idea of these topics. Less has been known about the enforcement of proclamations, and it is perhaps in this area that Heinze and Youngs have made their greatest contribution. Punishment seems to have been ordered most commonly in the locality where the offence was committed, by justices of the peace, mayors, or other administrative officers. The Council could commit offenders to prison but offered no machinery for judicial trial. During the period from 1539 to 1547, while the Statute of Proclamations was in force, the Star Chamber and Exchequer were particularly active in hearing cases based on the violation of proclamations, although there seems to be no evidence that the special tribunal created by the act was used or was regarded as being separate from the Star Chamber. These courts continued to be active, even under the Tudor queens, but the situation is complicated by the fact that in many cases offenders were charged with violating both a statute and a proclamation; we cannot be certain that jurisdiction would have been claimed on proclamation alone.

The account of enforcement thus leaves some important questions answered only vaguely or not at all. The problem is more apparent in Youngs'

book, for, while he lists an impressive body of court records which he has examined, he does not draw his information together into a single chapter, as Heinze does. In other ways, too, the organization of the two volumes is not parallel. To some extent the different periods require different treatment, but a closer similarity in organization would have made it easier for the reader to follow certain topics throughout the era. Heinze's "new look" at the Statute of Proclamations in fact provides little that is truly new, and the story remains much as Elton told it in his *English Historical Review* article of 1960. (On the question of Somerset's repeal of the Statute Heinze does offer an unusual, and probably correct, interpretation.) There is little in either book to show how ordinary subjects regarded proclamations; one wonders if Elton's story of a drunken mob in Coventry tearing down proclamations and using them to wipe excrement should be taken as a sign of widespread dislike. The interrelationship between statute and proclamation—Parliament and Council—is not explored very fully. The question of initiative is also dealt with rather superficially. Youngs' occasional forays into this area, for instance his fascinating account of the conflict between the Vintners' Company and the City of London which lay behind proclamations dealing with the price of wines, are so good that one is left wanting more. Youngs is occasionally vague on matters concerning religion, as when he speaks of Elizabeth restoring and revising "the Edwardian Prayer Book." There were of course two Edwardian Prayer Books; it was the second, of 1552, which became the Elizabethan model. Finally, there is no attempt to put Tudor proclamations in perspective by comparing them to those of other periods issued in England or to those promulgated by other sixteenth-century European rulers.

Some of these criticisms are petty. Others reveal major areas where we would like to know more. But on balance historians have reason to be grateful to Heinze and Youngs for these careful, judicious volumes. They set the record straight at many points and tell us as much about Tudor proclamations as we are likely to know for many years.

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LESLIE P. FAIRFIELD. *John Bale: Mythmaker for the English Reformation*. West Lafayette, Ind.: Purdue University Press. 1976. Pp. x, 240. \$9.75.

It is a shame that the material limited the scope of the originality of this study. We already knew of Bale the antiquarian and Bale the writer of moral-

ity and religious plays from Harris, McCusker, and Blatt. Naturally the book opens with Bale's background as a Carmelite and his conversion, necessarily traversing old ground. When it is done we are left with the impression that Bale was flattered by the prospect of aristocratic Protestant patronage, and had as well, an eye for the political future. He was, moreover, a temperamental extremist. This is not particularly revealing, but that is no fault of the author. He is at his best in these biographical sections, looking with care from many vantages, having a fine eye for place and for the rhythm of events. Indeed, I hope he turns his great skills to a large biography in future for the sensitivity and delicacy here displayed are rare.

Leslie Fairfield argues that Bale mediated for the English taste that long tradition which envisioned periods of Church history conforming to the seal openings of Revelations. Bale rejected millenarianism, but he absorbed the earlier analyses of periods and remade them to fit the pattern of English Church history. This work inspires the subtitle of the book: that he was a mythmaker for the English Reformation. His view, Fairchild argues, was passed on to Foxe and then to the wider reading public. In fact, while Bale may be credited with some influence on the 1563 edition of *Acts and Monuments*, Foxe altered his periods for the 1570 edition which was adopted later by Napier. The case, therefore, must remain slim.

Another claim is made for Bale as a mythmaker: that he created the first martyrs for the new Protestantism. He glorified one rebellious Lollard and gave a touching description of the trials of Anne Askew, a sacramentarian, burned under Mary. He proffered posterity his autobiography as well, perhaps as a saintly exemplar.

What Bale did was enlarge and enrich the old historical futurist exegesis of Revelations' seal-openings by drawing on his rich antiquarian knowledge. For this, and for his modest influence on Foxe, we must be grateful. Especially we should be grateful to Fairfield for his care, his caution, and his devotion to a difficult subject.

JOHN F. H. NEW
University of Waterloo

J. SEARS MCGEE. *The Godly Man in Stuart England: Anglicans, Puritans, and the Two Tables, 1620-1670*. (Yale Historical Publications. Miscellany, number 110.) New Haven: Yale University Press. 1976. Pp. xviii, 299. \$17.50.

When Bishop Hall published his *Humble Remonstrance to the High Court of Parliament* early in 1641, he correctly pointed out that the press had been dominated by the Puritan language, whereas the "orthodox part in this whole Realme" was "qui-

etly silent." This disparity, rather interestingly, has been reflected in modern historiography of the religious conflict in early Stuart England. Puritanism, as one author puts it, has produced "a minor scholarly industry"; studies of Anglicanism are, in comparison, far fewer in number. Furthermore, still fewer works deal with the Anglicans and the Puritans simultaneously and comparatively. In the light of this, J. Sears McGee's book is a well-conceived effort to redress the balance of modern historiography of religious thought in early seventeenth-century England and to present a comparative understanding of Puritanism and Anglicanism. And it is a well-written book.

The Anglicans and the Puritans shared a large common ground in their perceptions of man and God, conversion and salvation, sins and virtues. Nonetheless, McGee argues, there are essential differences between the two groups—so much so that "an Anglican could recognize a Puritan when he met one and vice-versa because each viewed the other from behind lenses strongly tinted by a particular ideal conception of true Christianity." According to McGee, the most trustworthy guide for distinguishing between the "two ideal types of the individual Christian man, one Anglican and the other Puritan" lies in their interpretations of and emphasis placed upon the Two Tables of the Decalogue. The Puritans placed their emphasis upon the First Table (Commandments I-IV, which define man's duties to God), whereas the Anglicans tended to lay their primacy upon the Second (Commandments V-X, which specify man's duties to other men). McGee convincingly demonstrates how this difference in emphasis upon the Two Tables led to unavoidable conflicts between the Anglicans and the Puritans in their attitudes toward certain fundamental issues in religion and society: the true worship of God and idolatry, obedience and peace, fellowship and charity, and, above all, the concept of godly man.

No short review can do justice to the rich scholarship of McGee's book. The comparative structure, the subtle analysis, and the clarity of presentation are only a few of its high qualities. One might expect McGee to have adopted a more dialectic approach in his treatment of ideas in history, but there can be little doubt that this is a worthy supplement to the tradition of American scholarship on seventeenth-century English religious thought. One minor oversight may be worth noting. Anthony Farindon, whose writings McGee uses extensively in this book, was not rector of St. Mary Magdalen, Milk Street, in the City of London, as his name does not appear in the parish records (London Guildhall MSS. 2596/2, 2597/1).

TAI LIU

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ANTHONY FLETCHER. *A County Community in Peace and War: Sussex, 1600-1660*. New York: Longman. 1975. Pp. x, 445. \$35.00.

Exploiting an enviable wealth of archival resources, Anthony Fletcher has written a richly-textured analysis of the administration, social conditions, and political and religious experience of Sussex from the accession of James I to the Restoration. This broad chronological framework is Fletcher's most significant contribution to Stuart local history. Recent works on the Civil War in the provinces have seldom provided more than an attenuated introductory review of the 1630s. Fletcher's expanded perspective provides a far more satisfactory account of the disruption that the war visited upon local society and of the continuities that survived both the conflict and the political experiments of the 1650s. The book also makes a number of specific additions to our understanding of provincial life in the seventeenth century. Some of these points stem from Fletcher's employment of previously established lines of inquiry, although his conclusions do reinforce the theme of the diversity of experience among the English counties which Alan Everitt has emphasized. In certain respects, however, in particular in his emphasis upon the ties of friendship around which the social world of the Sussex gentry revolved, his work breaks new ground and suggests directions for future research.

The book, then, is an important, multifaceted study, but it is not flawless. My most serious reservations stem from Fletcher's frequent, yet uncritical and opaque, evocations of the concept of "the county community." The phrase, as Fletcher employs it, lacks analytical consistency. Sometimes it refers to the dominant magnate elite of the shire, on other occasions to the total population. Most frequently Fletcher uses the phrase to refer to the Sussex gentry, but much of the information he provides suggests that no county-wide, cohesive group informed by a vital sense of its corporate identity existed in the county. Poor communications divided the gentry of the eastern and the western rapes. The two groups moved in distinct social circles, and local administrative arrangements reflected this bisection. In 1642-3 the eastern gentry actively embraced the Parliamentary cause, while their western counterparts edged into royalism or neutralism. The existence of a homogeneous "county community" hardly emerges from this history. Finally, Fletcher's infatuation with the nebulous "county community" concept leads him to argue that the gentry's political consciousness was forged exclusively in the local arena and was primarily concerned with the defense of local interests. Edward Alford's zealous defense of

parliamentary privilege in the 1620s was not, as Fletcher infers, based entirely "on his experience as a county gentleman," nor was the eastern gentry's 1641 commitment to the abolition of episcopacy solely a reaction to the timid and faltering efforts of Bishops Montague and Duppa to enforce ceremonialism: both were the products of political and religious controversies viewed in a national context.

CLIVE HOLMES
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J. S. KEPLER. *The Exchange of Christendom: The International Entrepôt at Dover, 1622-1651*. Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 191. \$17.00.

England's ability to capitalize economically on its neutrality during the Thirty Years War has received only limited attention. In a short, meticulously researched, highly technical, and overpriced study of the hitherto neglected international entrepôt at Dover, J. S. Kepler has in large part eliminated this deficiency. His book not only is significant for its insight into England's commerce during these years but also has implications for students of the foreign policy and finances of Charles I and the Long Parliament.

The entrepôt provided foreign merchants with safe English ships for their commodity traffic and the Spanish government with a means of shipping silver to Flanders to finance its armies. The English beneficiaries were the customs farmers, the government, and the merchant marine, half of which was employed in the re-export trade of the entrepôt during its heyday in the late 1630s. With thorough and faultless documentation, Kepler traces the beginnings of the entrepôt back to 1621, when the end of the twelve-year truce between Spain and Holland created a need for neutral carriers. But his most valuable observations concern the rapid expansion of business following the outbreak of war between France and Spain in 1635 and the subsequent steps taken by Charles' government to encourage and protect the entrepôt. The title on the outer cover of the book wrongly gives the cut-off date as 1641. Actually, Kepler provides a useful discussion of the continuing import of Spanish silver up to 1647 and of the efforts of the Long Parliament to maintain this traffic.

Scholars may find the book disappointing since it is an expansion of a previously published article and offers few fresh insights. Only one chapter (the least significant in the book) is wholly new. The only major new observation is the speculation that the entrepôt may have seriously damaged the export of England's New Draperies. Also, the book is as stolid as it is solid. The text suffers from long

quotations which are more distracting than illuminating. Nevertheless, the press deserves commendation for its willingness to publish original and highly specialized contributions to English economic and local history by young scholars.

JOHN T. EVANS
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JOHN MANNING WARD. *Colonial Self-Government: The British Experience, 1759-1856*. Buffalo: University of Toronto Press. 1976. Pp. viii, 399. \$25.00.

This excellent book disproves the notion that the constitutional and administrative history of the British Empire is a field long since exhausted, and all students of the subject will need hereafter to contend with the arguments put forth by John M. Ward. The thesis of this work of careful scholarship is that the Whig interpretation of colonial history has always been out of touch with reality, since the empire builders did not hold the views later historians argued they held. The notion of responsible government, which the older generation of historians maintained came out of a knowledgeable rethinking of the problem of colonial self-government as a result of the American Revolution, was in fact a theoretical construction after the developments had taken place. When Lord Durham went to Canada, he had no theory in mind; what he found convinced him that the Canadians were bent upon running the colony for themselves, and he concluded that since Britain could not prevent them from doing so, some rubric must be found to give Britain the appearance of having devolved power rather than having had it once again wrestled away. The principle he applied to Canada, and which he said he was bringing in from Britain—that a ministry must be able to hold a parliamentary majority on major issues, or resign—was not even established in Britain itself, much less within the thinking of anyone at Westminster who concerned himself about the colonies, but Durham nonetheless carried the day with his assertion of an unproven principle for the Canadians. In this sense, Canada was far less the Mother Dominion, Durham more the contriver of a means by which the empire might be held together. Later generations of historians turned what had been an ad hoc solution to a specific problem into a program of evolutionary constitutionalism and made Durham the opportunist into Durham the statesman.

The bare bones of this argument have been exposed before, and recently Ged Martin has shown that the Durham Report was not all that it has been taken to be. But Ward's sophisticated analysis of the situation both within Canada and at Westminster, his careful inquiry into how the idea

of responsible government took root in Britain after its enunciation for Canada, and his cautious and convincing shift of attention back from the periphery to the center of the empire make this a fresh and important book. Coming from an Australian scholar, the volume includes less material on Australia and New Zealand than one might expect, but there is more on the West Indies than is usual. The book's subtitle indicates best where the research, and the argument, are directed, and in this direction the author has sounded the loudest trumpet in a new historiographical revisionism.

ROBIN W. WINKS
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REED BROWNING. *The Duke of Newcastle*. New Haven: Yale University Press. 1975. Pp. xiv, 388. \$20.00

The Duke of Newcastle symbolizes the politics of patronage and connection which characterize the eighteenth century. He held major office for almost half a century. Yet Reed Browning has written the first full-scale biography. Part of the reason for this gap in the scholarly bookshelf is obvious—the length of Newcastle's career and the enormous volume of his papers would daunt most prospective biographers. Another explanation lies in the duke's character. It is easy to write of heroes and villains, of men whose strong personalities have stamped their image on an age. Only the most dogged of academicians, however, can be stirred by the prospect of devoting years of study to a man aptly described by Lord Shelburne as having "no resolution or mind of his own."

Browning does find redeeming qualities in the duke. If he was weak and vacillating, his motives were usually good. He was faithful to his wife, his friends, his social class. He "handled both foreign and financial affairs in a fashion we can call professional" (p. xiii). But Browning's narrative, based on extensive research, provides little support for any positive view of Newcastle. His foreign policy was often trivial, as in the abortive imperial election scheme; occasionally irresponsible, as in the events leading to the War of Jenkin's Ear; and even disastrous, as in the early stages of the Seven Years' War. His domestic policies, when he had any, were little better. Browning himself concedes that "ultimately he was a failure" (p. xi).

A number of faults mar this book. The style is cumbersome and the narrative frequently unclear and sometimes contradictory. Related to this problem of inconsistency is a tendency to overstatement. Historians will surely be surprised to learn that "only in the middle of the eighteenth century was it possible for a man like the duke, so

clearly unfit for the leadership responsibilities he now assumed, to reach the pinnacle of political power" (pp. 200–01).

Newcastle's career offers a unique perspective on several crucial topics in eighteenth-century history: the role of patronage, the implications for national policy of the close relationship between the Treasury and the City, and the changing nature of Whiggery. While Browning deals at length with this final point, his treatment of patronage is routine, and he says almost nothing about the "moneyed interest" and its dealings with the government. The result is a biography which clarifies much concerning Newcastle's private life and his electioneering, but does little to answer broader questions about either the man or his times. For insight into the duke one should still turn first to Namier's brief sketch in *England in the Age of the American Revolution*.

JAMES MCKELVEY
University of Connecticut

PHILIP ZIEGLER. *Melbourne: A Biography of William Lamb 2nd Viscount Melbourne*. New York: Alfred A. Knopf. 1976. Pp. 400, x. \$15.00.

Almost a quarter-century has elapsed since Lord David Cecil completed his celebrated biography of Melbourne. An evocative study of high society and an elegant portrait of an insouciant aristocrat, Cecil's biography was superficial in its treatment of politics. Anticipating the inevitable comparison, Philip Ziegler justifies his revisionist exercise by utilizing newer, valuable sources, among them the Brougham and Broadlands papers, some additional Holland House papers, and an autobiography of Melbourne's earlier years written in 1813. While cultivating the appearance of the equable amateur, Melbourne is now characterized as the calculating professional, "ambitious, cynical and almost wholly without political principle" (p. 9). Perhaps Ziegler was so mindful of Cecil's assessment as to fashion its opposite, for his interpretation is forced, hardly sustained by the evidence. Indifference becomes a virtue, political vacillation a talent.

Ziegler's imaginative sympathy and occasionally witty style serve well for the early and middle years. The world of young William Lamb is effectively presented, from his birth of questionable paternity in 1779, through the years at Eton, Cambridge, and Glasgow, and his entry into society. His older brother's death in 1805 made him heir to a peerage and enhanced his standing with the rump of Foxite Whigs. But Ziegler excels in depicting the personal life, the tempestuous marriage to Caroline Ponsonby, Lamb's painfully ambiguous situation in London society, his subsequent

affair with Lady Branden, and, the *cause célèbre* of the 1830s, Melbourne's exoneration while prime minister from the charge of adultery with Mrs. Norton. Revealed in the process is yet another aristocrat's attraction to the practices and fantasies of sexual flagellation.

Most of the book is properly devoted to Melbourne's political career, from his recruitment by Canning in 1827 as Irish secretary through the end of his second administration in 1841. Though there is little remarkably new, the practice of cabinet government, Melbourne's tutelage of the young Victoria, the Bedchamber controversy, and the Near Eastern crisis of 1839-40 are competently summarized. But the book lacks depth. To be sure, Ziegler has written a life of Melbourne not a history of early nineteenth-century England. Nevertheless, can one ignore the scholarship of D. C. Moore in evaluating the Reform Bill? Is the "revolution in government" controversy entirely irrelevant to a biography of a home secretary and prime minister in the 1830s? The appallingly complicated Irish question is barely revealed, Melbourne's virtual abdication of responsibility feebly justified. William IV's dismissal of the Whigs in 1834 and Melbourne's characteristic indecision in opposition to Peel's brief administration require extended analysis.

The craft of historical biography persists in England, the quality ranging from the potboiler to the rare reconstruction of history in microcosm. Though hardly an example of the former, Ziegler's *Melbourne* is far from attaining the enduring quality of the latter.

ABRAHAM D. KRIEGLER
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MICHAEL BARKER. *Gladstone and Radicalism: The Reconstruction of Liberal Policy, 1885-94*. New York: Barnes and Noble. 1975. Pp. viii, 308. \$17.50.

E. J. FEUCHTWANGER. *Gladstone*. New York: St. Martin's Press. 1975. Pp. x, 315. \$13.95.

M. R. D. FOOT and H. C. G. MATTHEW, editors. *The Gladstone Diaries*. Volume 3, 1840-1847; volume 4, 1848-1854. New York: Oxford University Press. 1974; 1975. Pp. lvi, 686; viii, 724. \$85.00 each.

It is a commonplace that great politicians are larger than life. But surely Gladstone is unique in the history of politics—unique in the length of time he commanded respect and subordination; in his phenomenal energy; in combining an instinctive reach for power with administrative capacity of astonishing detail; in his voracious intellectualism; in the symbiosis of religious passion, psychological torment, and sense of mission; in the long pilgrimage, nearly to the end of his life, to a radicalism

that only a prescient few had discerned as early as the 1840s in the rising young high-church Tory. No American politician is in the running. Churchill ranks with Gladstone on many of these heads but lacks the religious dimension and began rather than ended as a radical—an altogether easier course. Churchill, moreover, had a distancing humor that qualifies the intensity; like Disraeli or Lloyd George, he seems ultimately more human than superhuman. Bismarck accomplished more daring and significant works, with a comparable appetite for power, a like sense of mission, and perhaps worse inner suffering; but the intellectualism and the religious passion—and heaven knows the mission—were missing. De Gaulle perhaps. But politics is probably the wrong place to look. One is more likely to find Gladstone's equal among the saints of the Church—Augustine or Bernard—a notion that would have pleased the G.O.M., robbed as he was of a clerical career by his father's insistence.

The Gladstone industry was late in starting, but it is now in full production, and fortunately so. The resources are tremendous, and even in these times of high costs, scholarly and philanthropic institutions have combined to speed access to the materials. But how are historians to encompass it all? The three books here reviewed define the problem. Sir Philip Magnus' elegant biography appeared in 1954, and it is time for another single-volume attempt. Given the cruel restriction of space—fewer than 300 pages—E. J. Feuchtwanger has done an excellent job. Though he lacks the literary sparkle of his distinguished predecessor, Feuchtwanger writes well and clearly, has an admirable sense of balance, and is modest in his interpretive ambitions. The book gets better and better as it goes on; the mastery of the post-1868 chapters (in Feuchtwanger's own area of research) is greater than for the Peelite period; and at the end Feuchtwanger eloquently captures something of the grandeur of his subject. Inevitably in such a short book the public life predominates.

Michael Barker is not a biographer but a political historian concerned with the last years of Gladstone's leadership, from 1886 to 1894, when at last the prime minister freed his restive but cowed colleagues by resigning. Though in many ways Gladstone remained a Tory to the end, he responded with startling percipience to some of the radical impulses of the late century and, having foreclosed Chamberlain, in the end outdid him—one of the more delicious ironies in English history. Barker is rightly concerned to make this late radicalism apparent, but the form of his book (probably inevitably) prevents as fully concentrated a demonstration as one might like. Because radicalism was so fragmented in these years and

because political processes were undergoing such rapid transformation, Barker must devote much space to explaining context, and Gladstone disappears for pages on end. The result is a certain disjointedness: the book is, as it were, a series of articles strung together with Gladstone as the intermittently obvious common thread. J. B. Brebner liked to maintain that a title consisting of a name and a movement joined by a simple "and" showed that the author had not solved his conceptual problem; however that may be, more useful books like Barker's, putting Gladstone in his political setting, will be needed before the phenomenon of the man can be mastered. Again it is the public Gladstone we see, and the inner dynamics that account for this astonishing old age are not speculated upon.

The third and fourth volumes of the diaries are a different matter. Here the private life predominates. The extensive entries concern mostly illnesses of the diarist or his family, deaths, and foreign travels. It is a useful perspective to realize that the famous and crucial visit to a Neapolitan prison came at the very end of a three month stay in the city and that, while there are many indications of conversations with his chief informant J. P. Lacaita, Gladstone's time seems largely to have been taken up with looking at pictures and palaces, exploring bookshops, seeing (or rather not seeing) the liquefaction of the blood at San Genaro, and, above all, attending church. There are indications of letters written, committees attended, and memorandums drafted, but for the most part only telegraphic notations like "B.T. 12½-7" or "House 4¾-8¾ (spoke an hour, by desire) & (after tea at home) 9½-1" signal the public life that preoccupies historians. For revealing reflections on issues and incidents in his public life, one must go to the Gladstone papers, and the editor of the diary (H. C. G. Matthews replaced M. R. D. Foot in the early stages of preparation of these two volumes) has helpfully included extensive extracts.

The editing is extraordinarily impressive, not only in the identifying footnotes but in the imaginative appendices—"Where He Was," a list of dates and places that shows how steadily Gladstone was on the move (and what a godsend the railway was) and "Dramatis Personae," noting first references to all individuals seen or written to or about. But the full usefulness of the diaries will have to wait on the complete index to appear in the last volume and on the promised bibliography of all works read by Gladstone. The latter will be especially valuable, because one of the major impressions one retains from the diary is of the gargantuan appetite for reading, concentrated at times but mostly of astonishing miscellaneousness.

The chief new insight to be derived from these

two volumes concerns Gladstone's concern over his besetting sin, "impurity." Specifically, that meant a compulsion to read what he considered pornography and his recurrent lapses into sexual imaginings. In an intelligent and balanced discussion of this problem in the introduction, the editor (drawing on G. M. Young) points to Gladstone's Oxford-trained yet Evangelical-like need to analyze his transgressions, not merely by introspection but by abstraction and list-making. Matthew also makes it clear that the remedy—self-scourging—to which Gladstone resorted from time to time was less an instance of the "English vice" than a religious practice not uncommon in those years and practiced or contemplated by Newman, Pusey, and members of the "engagement," a small Tractarian society devoted to good works, to which Gladstone belonged in the forties. Gladstone's involvement in the rescue of prostitutes was an ambiguous weapon in this struggle; he found hope of purging his own sinfulness by helping the only less sinful, but he found evident stimulation in the encounters as well, thus redoubling the difficulty.

It is too easy, in our intermittently uncensorious age, to be astonished at the restraint and its psychological cost or to imagine a simple solution like taking a mistress. It is even easier to invoke that tired cliché of Victorian hypocrisy or, worse, to find a prurient pleasure in unmasking the great. In a time in which we have been treated to continual revelations about the sexual adventures or misadventures of politicians on both sides of the Atlantic—with an avidity that, on balance, makes the censoriousness against Parnell and Dilke seem almost appealing—Gladstone raises some serious questions in a way that, one hopes, compels seriousness in response. That politicians or other hard-driving, egocentric, achieving men have notable sexual appetites is part of folklore, with much evidence to support it; that frustration, sharply altered circumstances, or even notable accomplishment may trigger that appetite is well-known—Gladstone's difficulties increased markedly following the Maynooth resignation and in the course of his realization that his ideals of a religiously purified state and of a Tractarian Anglicanism were being falsified. But what is one to make of the element of risk-taking that seems so much a part of Gladstone's nocturnal expeditions? Here is a good, precise subject for a psychohistorian willing to abjure the metahistorical or metamoral preoccupations of so many of them.

The diaries emphasize the magnitude of the task faced by historians who contemplate this heroic figure. The shape of Gladstone's public life and his immense grasp on three generations of his countrymen cannot rest much longer on mere citations

of outrages witnessed or on formulaistic invocations of religious commitment or a European sense. From his reading, musings, devotions, temptations, and sufferings will have to emerge a new appreciation of the sources of his strength and his accomplishment.

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DENNIS F. THOMPSON. *John Stuart Mill and Representative Government*. Princeton: Princeton University Press. 1976. Pp. vii, 241. \$13.50.

This book is especially welcome since Mill's *Considerations on Representative Government* has never undergone a critical examination in its own right, though bits and pieces have been used to lend support to a variety of interpretations on his political thought. Consequently, because Dennis Thompson's analysis is clear, systematic, and thorough, this book should lay to rest many of the misinterpretations that have been current about Mill's theory of government, which, as the author solidly argues, is a theory of democracy. One of Thompson's aims is to assess Mill's theory in the light of recent studies in the social sciences and in democratic theory. Thus, in confronting a number of the critics who have emphasized either elitism or inconsistency in Mill's theory, Thompson proves that while "Mill does not qualify in modern terms as a full-fledged participatory democrat," he cannot be considered an elitist democrat either. Noting that "most interpreters" emphasize Mill's mistrust of social equality, Thompson distinguishes at least three favorable aspects of social equality discussed by Mill in *CRG* and expanded upon in *The Subjection of Women*. Acknowledging that Mill did not provide sufficient scope for citizen involvement in the political process, Thompson's analysis shows clearly that Mill's institutions fail to provide the means by which an educated elite would hold real political power as several critics have mistakenly charged.

Yet Thompson's principal objective is to explore the structure of Mill's theory, which, he argues, is coherent and systematic, though not totally free of "internal tensions." His strategy in the first two chapters is to analyze the major components of Mill's theory: the propositions which comprise the arguments Mill used for the two basic principles of participation and competence which form the core and cornerstone of his theory. Here Thompson shows how Mill's enthusiasm for extensive citizen participation in the political process is grounded in his protective argument: each person is the safest guardian of his own interests; and in his educative argument: involvement in politics develops a more

knowledgeable and public-spirited citizen. In his advocacy of the principle of competence Mill again employed the protective argument: democracy could safeguard itself against the dangers of mediocrity by giving much weight to the deliberations of superior intelligence; and the educative argument: the influence of the competent minority was an important element in the political education of the citizenry.

From here Thompson moves to an examination of Mill's electoral and political institutions and rightly argues that Mill never sacrificed either of these principles. Indeed, Thompson's interpretation of the most controversial parts of *CRG*—plural voting, personal representation, the role of the representative assembly—shows that Mill's analysis of the tension between the two principles and the political solutions offered by him were attempts to resolve the potentially conflicting demands of an enlarged electorate and an educated minority. Yet in his attempt to harmonize participation and competence, Thompson finds that Mill did not precisely define the necessary balance between the two, especially when courses of action had to be chosen that would further the values of one over the other. Last, Thompson shows how Mill attempted to further the goals of participation and competence in his theory of development by which Mill sought to establish the conditions necessary for social change and the improvement of democratic government in the future. This was the weakest element in Mill's theory, but Thompson attributes this to its ambitious aims and Mill's willingness to grapple with difficult problems.

I regard Thompson's work as a major contribution to Mill scholarship; it has disposed of many problems which have long perplexed interpreters of Mill's political thought. It is done well, in clear, polished prose, with consistency of manner and tone. *CRG*, with appropriate support from additional writings of Mill drawn on by Thompson, has, at long last, received the attention it deserves.

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ALAN D. GILBERT. *Religion and Society in Industrial England: Church, Chapel and Social Change, 1740-1914*. (Themes in British Social History.) New York: Longman. 1976. Pp. ix, 251. Cloth \$15.50, paper \$8.50.

This book is at once a model for the application of sociology to the history of religion and an important contribution to the monographic literature. It demonstrates that the rise of evangelical Dissent, which by 1840, had come within a few percentage points of rendering the Church of England a minority religion, constitutes a single coherent histor-

ical phenomenon susceptible to sociocultural analysis. Alan D. Gilbert shows that the Establishment was ill-equipped to meet the challenges of the Industrial Revolution. The parish system, suitable to a church with universalistic claims was insufficiently flexible to compete in a pluralistic religious climate; moreover, it was rent by the accumulated abuses of centuries. Anglicanism was intimately tied to a landed class whose power over significant segments of the population was waning. At the same time, new forms of production fostered the growth of those strata of society, primarily artisans and the "lower middling," who were least susceptible to the prescriptive sanctions of the Church and from whose ranks Dissent drew a disproportionately large percentage of its membership.

Between 1740 and 1840 the denominations of evangelical nonconformity took advantage of the Church's weakness. They grew, not by competing with the Establishment directly, but by bringing religion to where it was weak, for example, to large parishes with unattended hamlets, to parishes where there was no single large landowner, to new extraparochial settlements, to mining and manufacturing villages. As Anglican norms became increasingly outdated for large numbers of people, the Methodists and New Dissent helped legitimate protest against the old order, supported individual self-improvement and mobility, as well as providing a defense against the anomie of the new society.

By 1840 the spectacular growth of all the denominations of evangelical Dissent came to an abrupt halt. While their absolute numbers continued to increase until World War I, their share of the adult population ceased to grow around the beginning of Victoria's reign and actually began to decline in the 1870s for a number of reasons. First, the Church reasserted itself during these years, according to Gilbert, by acting as a *de facto* denomination—as one among several legitimate religious bodies which has to compete for members in a pluralistic society. Absenteeism and other abuses of the parish system were ameliorated, the laity was given a greater role in local affairs and the village church came to provide the kinds of social services that dissenting sects had long offered. Second, as factory labor became the dominant form of industrial production, the size of the groups from which Dissent recruited began to decline. Finally, over time, the dissenting sects lost their proselytizing zeal, denominational barriers sprang up, the ministry grew more distant from the laity, and the social status of membership rose, thereby further deterring the allegiance of the masses. By 1850 the Establishment and Dissent had assumed similar positions on the sect/church continuum.

Ultimately, however, as Gilbert argues, the intense religious climate of the Victorian era was but an Indian summer. A crisis of plausibility, first perceptible among the urban working class, engulfed both Church and Dissent, although the latter was especially vulnerable once its political program had gained success. Industrial society, Gilbert suggests, is fundamentally secular in its world view.

The book's only limitation is that by concentrating on the Church and various denominations, it gives short shrift to religious phenomena which transcend these boundaries. What, for example, are the sociocultural foundations of evangelicalism within the Church? Do Anglican conversions vary from those in Dissent? Gilbert's concentration on common growth trends blur regional and temporal variations. How, for example, can he account for the fact that Wesleyanism did so badly in Lincolnshire during the eighteenth and early nineteenth century and then suddenly met with astounding success in succeeding decades. Finally, no sociocultural history of religion can be fully satisfying. Conversion, as he would admit, is clearly more than a palliative for anomie; differences in religious style and theology more than mirrors of social change.

Still, Gilbert has written a lucid, provocative, and far-ranging book of interest well beyond the circles of British history. Specialists should turn to it for its massive statistics, impressive documentation, detailed arguments; others should read it for its methodological insights, and some perhaps will seek in it clues to the role of religion in the world today.

THOMAS LAQUEUR
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IAN BRADLEY. *The Call to Seriousness: The Evangelical Impact on the Victorians*. New York: Macmillan Publishing Company. 1976. Pp. 224. \$12.95.

The significance of Evangelical Christianity for nineteenth-century Britain has been acknowledged by Victorian scholars since Halévy's day. Biographies of leading Evangelicals like William Wilberforce and Lord Shaftsbury have existed for many years and monographs exploring theological and social characteristics of Evangelicalism continue to be written. To date, however, the single interpretative work has not been produced.

Ian Bradley has attempted to remedy this situation but has achieved only partial success. He outlines the basic theology of Evangelicalism—the conviction of personal sin, of Divine grace and the assurance of salvation for believers. The basic theme of the book is the overwhelming sense of

duty to God and man which weighed so heavily on many a Victorian mind. Bradley's sources include biographies, letters, books, tracts, and volumes of devotional literature. He has also made use of Victorian critics of Evangelicalism. The result is a very readable introduction to Evangelical programs for the spiritual and social regeneration of Victorian England. Bradley relates the plans for converting the urban masses, for philanthropy, and for the extirpation of vice at home and paganism abroad. Clearly the most important duty accepted by Evangelicals was the rearing of their children to lives of serious calling. While he deplores the rigidity, the oppressiveness, the inevitable cant and hypocrisy of Evangelical Christianity, Bradley does generally approve of the Evangelical impact on society. The Victorian middle classes especially were not frivolous people but serious pilgrims in a society strained by the consequences of industrialism, a nation which certainly profited by reform of its manners and institutions.

Bradley does not unfortunately go far beyond his illustrative material. He really does not explain how "vital" religion emerged from eighteenth-century laxness; he does not effectively portray the complex interplay between religion and the evolution of Victorian society. Evangelicalism was a social movement with a leadership, institutions, and a defined set of goals. Here is a phenomenon which lends itself to possibilities for serious socio-historical analysis which Bradley does not present. Although this book will be a considerable help to nonspecialists in Victorian history, it must be stated that we have not yet been given a provocative explanation of the role of Evangelicalism within Victorian society.

LEE E. GRUGEL
Moorehead State University

PAUL MISNER. *Papacy and Development: Newman and the Primacy of the Pope*. (Studies in the History of Christian Thought, number 15.) Leiden: E. J. Brill. 1976. Pp. vii, 204.

John Henry Newman's life-long desire was to establish a firm ground of authority for religious belief. Although he belonged to a generation that was rapidly drifting toward secularization of thought at every level, Newman strove to stem the tide. He went to Oxford as a mild Evangelical at a time when the traditional link between Tory church and Tory state was crumbling under pressure from liberal reforms. The influence of his new associates, Hurrell Froude and John Keble in particular, led him to a High Church position. The High Church tradition, looking back to the patristic age for inspiration, had once held a respected

place within the Church of England, but by the beginning of the nineteenth century it was largely ignored.

From 1832 to 1842 Newman labored to bring the Church of the Fathers alive in England. This was the thrust of the Oxford Movement under his leadership. The ancient Fathers—Athanasius, Basil, Chrysostom, Gregory—would save the Church of England from disintegration. But the *via media* he so skillfully constructed—a theory of a middle Church somewhere between popular Protestantism and Roman Catholicism, avoiding the errors of both—came to be seen as a creation existing only on paper. Reluctantly and painfully he was driven to join Rome. His theory on the development of Christian doctrine was worked out to support his belief that the Church of Rome was the only legitimate continuation of the Church of the Fathers.

Paul Misner's study seeks to trace the working out of Newman's theory as applied to the primacy of the office of the papacy. He follows the process of Newman's thinking from his Oxford years, through the stormy Vatican I period which saw the controversial definition of papal infallibility, to the preparation of a final revised edition of the *Essay on Development* in 1878. He concludes with an assessment of Newman's place in the history of ecclesiology.

Misner's study is an exact one with a careful attention to detail. His scholarship, within defined limits, is impeccable. Yet a reader interested in the evolution of the papacy will be disappointed by the slight attention he gives to social and political factors. It is evident from some brief comments that he is aware of a curious one-sidedness in Newman's approach. By remaining within a confined theological context in his writings on the papacy Newman guaranteed that they would appear as a part of a radically conservative tradition.

H. A. MACDOUGALL
Carleton University

FRANCIS HILL. *Victorian Lincoln*. New York: Cambridge University Press. 1975. Pp. ix, 341. \$28.00.

Victorian Lincoln completes Sir Francis Hill's study of Lincoln that began with the publication in 1948 of *Medieval Lincoln* (the other volumes are *Tudor and Stuart Lincoln* [1956] and *Georgian Lincoln* [1966]). Like its predecessors in the series, *Victorian Lincoln* relies upon private papers and diaries, official records and newspaper accounts, and other scholarly and mnemonic studies. It is densely written, frequently colorful, and tastefully produced; it could easily serve as a model for those of us who yearn someday to write the detailed history of home environs to which we are deeply attached.

Inevitably, Victorian Lincoln swarms with conspicuous people: Colonel Sibthorp and the elder Charles Seely, long active in Lincolnshire and parliamentary politics; prominent Chartist Thomas Cooper; the first Lord Yarborough, who bullied his tenants into fox-hunting and who still lives on in the complaints of disgruntled bridge players; Joseph Ruston, an engineering wizard at age twenty-two who eventually propelled his foundry into an international supplier of farm equipment, locomotives, and road rollers; Francis Jonathan Clarke, four-term mayor of Lincoln and purveyor of Clarke's Blood Mixture, who was instrumental in effecting significant housing and road improvements; Christopher Wordsworth (nephew of William) and Edward King, guardians of the magnificent minster as bishops of Lincoln—the one naïve, relentlessly principled, and anti-Romanistic, the other eminently venerable (“he looked as if he had stepped out of a stained glass window”) but scandalously ritualistic.

Of equal interest is the emergent picture of Lincoln, at first pastorally insular, lurching painfully into—and often out of step with—energetic participation in the world community and the national movement toward multiform amelioration. Geographical isolationism was overcome with the coming of the railway at mid-century, but only after lengthy political and economic factionalism that served to impede progress. Cultural parochialism was even more vigorous: although local authorities had been empowered to provide a public library since 1850, Lincoln had none until 1891; there was no public museum before 1907. In regard to the public weal, one example must suffice: interest in improving sewage disposal and the drinking-water supply, sparked by the cholera alarm of 1831, flagged markedly after the panic subsided; the problem was not solved until 1911, seven years after one hundred deaths from typhoid fever shocked the community into renewed awareness. Internecine squabbles and the reluctance to spend public funds on humanitarian necessities all too often characterized Lincoln life during the past century. In this respect, some of us may think, *Victorian Lincoln* proposes another parallel between the Victorian and modern minds.

LESLIE G. BAILEY
Saint Martin's College

ROSANNA LEDBETTER. *A History of the Malthusian League, 1877-1927*. Columbus: Ohio State University Press. 1976. Pp. xxii, 261. \$12.50.

This monograph details the history of the first organization “to advocate voluntary family limitation, as a solution to the problems of . . . poverty.” There are two key words here: organization and

poverty. Rosanna Ledbetter's story tells of the internal struggles of a handful of personalities who led the Mathusian or Neo-Malthusian League. The history is sewn with bits and pieces from the lives of Charles Bradlaugh (better known for his association with the Free Love Movement), Annie Besant, Dr. H. A. Albutt, and of course the Drysdale legacy—starting with George (author of the *Elements of Science*) and ending with C. V. Drysdale, who remained an active evangelist decades after the organization was defunct. As the author is careful to note, advocacy of birth control was not new in 1877, but this was the first formal group to advocate the cause.

What made the League unique lies with the second word, “poverty.” For as one reads through Ledbetter's narrative (which is filled with digressions) one finds that this was the major concern of the League. The organization was founded upon the principles of Thomas Malthus and later of Charles Darwin. Birth control was seen as only a means to an end: the end of poverty. So what is distinctive? Poverty was of growing concern to many at this time—witness the works of the Fabians, the socialists, and the Labourites. While it would appear there would be positive reinforcement between these groups and the Malthusians, there was in fact direct conflict. While many Fabians, socialists, and Labourites recognized the need for family planning, they saw other goals as equally important in the battle against poverty, for example, better wages and education. The Malthusians opposed any such efforts to improve the standard of living (they were even opposed to free school meals). These measures they felt, encouraged overpopulation, the major cause of poverty.

As Ledbetter points out in her introduction, the debate continues today. It is an important point of controversy, but it gets lost in Ledbetter's minutiae (for example, the menu and guest-list of the fiftieth anniversary dinner of the Bradlaugh-Besant trial). The book is badly organized and the narrative tedious and distant. The importance of the League and its success cannot be measured by the limited demographic statistics that Ledbetter uses. It is more important to ask why the philosophy is still popular among certain sectors when it has been proven wrong.

PATRICIA BRANCA
Carnegie-Mellon University

NICHOLAS MOSLEY. *Julian Grenfell: His Life and the Times of His Death, 1888-1915*. New York: Holt, Rinehart and Winston. 1976. Pp. x, 275. \$12.95.

Julian Grenfell was the son and heir of Lord Desborough, and the grandson, on his mother's side, of Lord Cowper; he was educated at Eton and

Balliol, entered the army, rose to the rank of captain, was awarded the DSO, and was killed in action. He was also a poet. Nicholas Mosley (Lord Ravensdale) is the son and heir of Sir Oswald Mosley, Bart., and the grandson, on his mother's side, of Lord Curzon; he was educated at Eton and Balliol, entered the army, rose to the rank of captain, and was awarded the Military Cross. He is also a novelist. The point should be apparent: author and subject are virtually doubles, except for their dates. Grenfell was born in 1888, and died in the First World War; Mosley was born in 1923, and survived the Second.

Because this similarity exists, this book is not a conventional biography, but rather a meditation on the class to which both Grenfell and Mosley belong, on the power within that class of money, position, and social convention, and on the destructive instinct that impelled that class toward war in the early years of this century. Grenfell is offered as a historical figure, representative of the best of his generation; but he is also another kind of figure, a tragic hero of great gifts and promise who was destroyed by his class, and most of all by his mother, the most powerful representative of that class in his life. The mother-son relation is the core of the book, and it is Mosley's thesis that this destructive relationship can be seen as symptomatic of the entire Edwardian upper class, and Grenfell's death in war as the class' death-wish realized. This *liebestod* theory of the origins of the war will surely seem too simple to most historians; nevertheless it does isolate and dramatize a psychological aspect of the war that is otherwise hard to deal with.

Mosley's principal source of information has been the private papers of Lady Desborough, and this is obviously a distorting factor—like asking Jocasta to write the life of Oedipus. Her version of her son's life was biased, and on occasion intentionally falsified to preserve her own conviction of his devotion to her. Since there is not much other evidence to go on, Grenfell remains a somewhat blurred, though attractive figure.

Another source of blur is Mosley's strong sense of identification with his subject: "It must be clear to anyone who reads this book," he writes in a digression in mid-volume, "that I have some love for Julian Grenfell." This identification is so insistent that sometimes one cannot be sure whether it is Grenfell or Mosley speaking, Grenfell's Eton or Mosley's Eton, Grenfell's war or Mosley's war being described.

Nevertheless, the book is valuable for many reasons. It is a useful corrective to the common, or Wilfred Owen, view of the First World War soldier: Grenfell *loved* war. It is an intimate account of Edwardian upper-class life. And most of all, it is

an attempt at a psychology of the upper classes. No one else could have written it so movingly, or with such authority.

SAMUEL HYNES
Princeton University

BRITON COOPER BUSCH. *Mudros to Lausanne: Britain's Frontier in West Asia, 1918-1923*. Albany: State University of New York Press. 1976. Pp. 430. \$30.00.

This is the third volume Briton Cooper Busch has published in a decade on British middle-eastern policy. As with *Britain and the Persian Gulf, 1894-1915* and *Britain, India, and the Arabs, 1914-1921*, this study is based on British public and private archives that only recently have become available. Similarly, the interpretation emphasizes India's impact on British policy-making. Readers familiar with Busch's earlier works will again appreciate his clear, understated prose, the useful maps, and the footnotes that demonstrate his knowledge of relevant memoirs and recent monographs.

Busch links postwar Turkey, the Caucasus and Caspian regions, Persia, Afghanistan, and India in order to show how events in one area affected British policies in another. He uses the incongruous metaphor of tidal waves that move not only from west to east, but also from east to west. While he notes London's exaggerated fears about the area falling into chaos and Bolshevik intrigue in turn leading to the collapse of British rule in India, while he avoids getting caught in an all too familiar geopolitical linkmanship, and while he appreciates the restrictions increasingly imposed upon the exercise of British power in the Middle East after the war, Busch takes the War Office to task a few times for doing too little, too late. On the whole, he realizes that the soldiers faced awful assignments in their efforts to meet London's confused expectation and to cope with inchoate situations on the spot.

In filling out the British middle-eastern picture more fully, Busch makes some interesting observations. The British in India appear to have been in touch with postwar realities sooner than policy-makers in London. Still, allowances have to be made for the more complex interests London had to harmonize—including those of the Allies, in whom India traditionally put little stock. Besides seeing the old India hands in a new light, Busch suggests how soiled Foreign Office hands became with their illusions about Russia and the Bolsheviks, U.S. willingness to take on tasks in Western Asia that the British could not handle, and Italy's being put in charge of the Caucasus! Busch's connections between the Khilafat movement in India and the crisis in Turkey seem somewhat overdone, but he is correct in stating that Atatürk, Lloyd

George, the Arabs, and Greeks have so dominated historical consideration of Anglo-Turkish and inter-Allied postwar tensions that what he calls Britain's frontier in West Asia should not be ignored.

ROGER ADELSON
Arizona State University

N. H. GIBBS. *Grand Strategy*. Volume 1, *Rearmament Policy*. (United Kingdom Military Series. History of the Second World War.) London: Her Majesty's Stationery Office. 1976. Pp. xxvi, 859. \$60.00.

In his comprehensive study of British rearmament before the Second World War N. H. Gibbs undertakes to describe the interrelationship between the government's rearmament programs, the military's strategic planning, and the evolution of appeasement as the dominant theme of Britain's foreign policy in the thirties. After briefly surveying the extent and ramifications of Britain's disarmament in the wake of the First World War, the author concentrates on the development of the government's rearmament policy during the period beginning in 1932 with the cancellation of the Ten Year Rule (the assumption for the purposes of military planning that the nation would not become engaged in a major conflict within the next ten years), and ending in 1939 with Britain's declaration of war against Germany.

The government's decision to build a powerful air force and maintain her traditionally strong navy while neglecting the army is traced through the papers and minutes of the Chiefs of Staff, the Committee of Imperial Defence, and the Cabinet, as well as their subordinate committees, and Gibbs clearly delineates how this decision impinged on both strategic planning and the conduct of Britain's foreign policy. Considerable time is spent outlining the emergence of the specific programs of the three services, as well as detailing the strategic and diplomatic aspects of the major foreign policy crises of the thirties.

To a large degree the strengths and weaknesses of this book stem from the fact that it is part of Britain's official history of the Second World War. While it is the first volume in the history's military series, it is the last to be completed. The opening to scholars of the official documents on which it is based has freed Gibbs from the annoying and burdensome conventions under which the authors of the earlier volumes had to labor when those documents were classified. For example, he has been permitted to make specific footnote references to the documents he uses, and to identify individual ministers and civil servants by name rather than cloak them in the rather transparent anonymity of their titles as the authors of the earlier volumes

were forced to do. The easing of these conventions makes this volume both more useful for scholars and interesting for general readers than the earlier volumes of the series.

The primary weaknesses of this first volume of the *Grand Strategy*, its avoidance of controversy and its lack of analysis, are to a large degree attributable to the fact that it is part of the official history. An example of the first problem is the impression of almost complete harmony within the government that is given by Gibbs' description of the development of the rearmament plans, programs, and policies. The active competition among the three services for programs and funds is almost entirely glossed over as are the intense and often bitter struggles between the armed services, the Foreign Office, and the Treasury. Consequently little sense of the politics of policy-making within the government is conveyed.

Of greater significance is Gibbs' reluctance to analyze the financial and production limitations placed on rearmament by the government, limitations which he acknowledges were the determinants of the rate at which Britain rearmed. In a single chapter he briefly describes the emergence of the government's rationale for those limitations, but he specifically draws back (p. 316) from examining either the economic theory or the social and political assumptions underlying those limitations. While it is understandable that economic and political considerations might normally be beyond the scope of the official military history series, in this case those considerations dictated the extent and rate of British rearmament as well as the strategic planning and foreign policy based on it. Thus one would expect that they would be extensively analyzed. For an examination of the way those economic and political considerations shaped British rearmament see the reviewer's *British Rearmament in the Thirties: Politics and Profits*.

These criticisms notwithstanding, volume 1 of the *Grand Strategy* provides a comprehensive description not only of the development of British rearmament policy in the thirties, but of the strategic plans and foreign policies that stemmed from it.

ROBERT P. SHAY, JR.
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SIMON NEWMAN. *March 1939: The British Guarantee to Poland, A Study in the Continuity of British Foreign Policy*. Oxford: Clarendon Press. 1976. Pp. viii, 253. \$14.25.

Simon Newman's closely-woven study of the British decision to guarantee Poland's independence in 1939 interprets that fateful episode as the logical

culmination of a policy which the National Government had followed continuously since Hitler's coming to power. The author argues that the British never seriously doubted (as perhaps they should have done) that Germany's ascendancy in Eastern and Southeastern Europe was incompatible with their own survival as a European great power; hence they never really contemplated giving Hitler a "free hand" in the East, for that would have overturned the existing balance of power to their permanent disadvantage. Down to 1939 British policy concentrated instead on "resistance short of war" and took primarily economic and financial forms, supplemented by occasional talk of colonial concessions. This policy reflected Chamberlain's belief in the paramount role which the British economy would have to play in peace or war and a general conviction that military combinations against Hitler, made in the name of deterrence, would only bring war closer.

After the German occupation of Prague, which seemed to show that Hitler's appetite was insatiable, Halifax emerged as the chief formulator of policy and finally committed the British to the defense of Poland on the grounds that to do less would open Germany's way to the peaceful conquest of Eastern Europe, thus nullifying the West's economic leverage on the Continent and irreparably damaging its standing with the United States. All this was done in an atmosphere heavily freighted with feelings of humiliation, uncertainty, and incipient panic; and it may, Newman concludes, have been done for invalid reasons, because it is still not established whether Hitler meant to compass the ruin of the British Empire.

On its salient points the book is consistently persuasive and often convincing. Even so one may wonder whether Newman has not made too much of what he calls the "middle course" between unilateral concessions ("appeasement") and a sturdy defensive coalition (the "Grand Alliance"), for as he himself admits, the economic gambits on which it was based frequently had an *ad hoc* and very tentative quality. Similarly one may doubt whether he has got the emphases exactly right when he insists that the Cabinet knew for a certainty in March that its guarantee would have no deterrent effect on Hitler and amounted, therefore, to an anticipatory declaration of war. Several of the book's themes and conclusions have been foreshadowed elsewhere, but the controlled orchestration of the whole makes this an independently valuable contribution to the on-going appraisal of British policy during the last period in which the deliberations and decisions of Whitehall swayed the destinies of entire continents.

DONALD LAMMERS
Michigan State University

ELISABETH BARKER. *British Policy in South-East Europe in the Second World War*. (Studies in Russian and East European History.) New York: Barnes and Noble, 1976. Pp. viii, 320. \$27.50.

In recent years a number of studies have appeared which provide a much better understanding of the tumultuous events in Southeast Europe during World War II. Elisabeth Barker has made use of recently opened British archival material to present a detailed though narrowly focused examination of Britain's policy in the Balkans as devised and implemented by the Foreign Office and SOE. While the work covers British policy toward all the states from Hungary to Greece, the author devotes much more space to countries other than Greece and Yugoslavia.

Britain's interests in the area have traditionally focused on the periphery in places such as Greece and the Straits. During the interwar era Britain tended to follow the lead of France in its relations with the governments of the Balkan states. The outbreak of war forced the British to attach greater significance to the Balkans as a means of demonstrating their ability to carry on the struggle in Europe at a time when they were practically shut out of the continent by Hitler. Their efforts at bolstering the resistance of the Southeast European governments to Nazi blandishments down to 1941, were, as the author clearly demonstrates, inhibited by both a lack of resources and the fear of provoking Hitler and thus achieved little success. A clash with the Axis forces in this area was inevitable as Hitler shifted his strategy from a direct attack on Britain to one of dealing her a blow in a "peripheral" place like the eastern Mediterranean.

The author notes Churchill's desire to use the Balkans militarily to attack the Nazis as well as his concern to keep the Russians from acquiring a predominant position in the area after the war ended. She then illustrates in detail the divergence in outlook between the various policy and action branches of the government that made British activities in this area much less unified than many have presumed them to have been. Barker also mentions the differences of opinion between the British and Americans over policy concerning the Balkans but does not fully explore the problem.

As the war progressed and the balance tipped in favor of the Allies, the British were faced with the unwelcome though not unexpected fact that the Soviet Union was now the dominant power in the area. Whatever assurances the British had given to the leaders of the Southeast European states were negated by the fact that, except for Greece and to some extent Yugoslavia, Britain's presence was limited to propaganda, some military missions, and a far from perfect intelligence network. Barker

argues persuasively that the British, acting on the basis of political realism, recognized the certainty of Russian dominance in Romania, Bulgaria, and Hungary by the summer of 1944, and accepted this as an accomplished fact in return for retaining a measure of influence in an area of real interest, Greece.

The author's choice of a year-by-year and a country-by-country approach results in a large number of chapters (twenty) and inescapable fragmentation and repetition in the narrative. A third of the text is devoted to the period from 1939 to 1941 and the rest to the years 1941 and 1944. An annoying though small number of typographical errors (half a sentence is missing on page 230) do not significantly detract from the considerable worth of the book.

GERASIMOS AUGUSTINOS
University of South Carolina

PHYLLIS AUTY and RICHARD CLOGG, editors. *British Policy Towards Wartime Resistance in Yugoslavia and Greece*. New York: Barnes and Noble, in association with the School of Slavonic and East European Studies, University of London, 1975. Pp. xii, 308. \$27.50.

This book is a collection of articles based on material delivered at a conference at the University of London in 1973. Most of the contributors were participants in the events they describe. Elizabeth Barker's excellent piece on the foreign policy of the Foreign Office toward Yugoslavia concludes that, in priority, the Foreign Office was more interested in protecting the area from postwar Communism than in defeating Germans. F. W. D. Deakin exposes "The Myth of an Allied Landing in the Balkans . . ." as an American obsession, in a piece that supports the official histories of his British colleagues, Michael Howard and John Ehrman. Barker and Deakin are professional historians in the best sense but that is not true of Bickham Sweet-Escott and S. W. Bailey, former SOE agents who seem imbued with what Barker elsewhere called "a sort of T. E. Lawrence complex" (p. 30). Bailey was parachuted to Mihailović's headquarters as Senior British Liaison Officer. His article, "British Policy Towards . . . Mihailović," carefully documents Mihailović's collaboration with the Nazis, but his expressed purpose is to plead for a greater sympathy for the behavior of the Chetniks.

Three articles on Greece by Brigadier E. C. W. Myers, C. M. Woodhouse, and Richard Clogg are more thematically integrated. The three authors agree that the Foreign Office and Churchill were responsible for the Greek catastrophe by their insistent support of the king's determination to re-

turn to Greece before a plebiscite on the monarchy. All three agree that the critical point of no return was passed in September 1943 with the politically disastrous visit to Cairo of six Resistance representatives from occupied Greece. When the delegates insisted on discussing the composition of the Greek government-in-exile, Ambassador Leeper curtly terminated their visit and ordered them to return. The Foreign Office supported Leeper. The stunned delegates now became convinced that "British policy" was determined to restore the monarchy. This sad September moment fatally set events into motion toward their sanguinary dénouement: the British intervention and the bloody suppression of the resistance in the Battle for Athens in December 1944.

But the three authors differ as to why the Foreign Office should have changed its policy. To Myers, Communism was a false issue used by the Foreign Office in order to disguise its primary policy of restoring the monarchy. Woodhouse, on the other hand, has been persistently a cold war warrior. Nevertheless, he now concludes that the Greek Communist Party (KKE) was divided in counsel, unsure of its policy, and willing to seek an accommodation with the British. It was the insensitivity or lack of knowledge of these conditions, or both, on the part of the Foreign Office which led to the tragedy of the December Days. In other words Woodhouse has moved considerably in lessening the KKE's responsibility for the fighting in Athens. Clogg argues that, had the British lessened their support of the king they would have deprived the KKE of its strongest appeal which was to the republican sentiment of "ninety percent" of the population. Thus at the root of Clogg's argument one finds, again, the proverbial fear of Communism as the *deus ex machina*. These differences are most important because the premises behind them measure the inadequacy of Greek resistance studies to date. What the three really disagree about is how best to restore the state, and they refuse to understand the Greek resistance as a national and revolutionary movement.

This collection underscores the importance of the new documentation in the Public Record Office but it does not offer any new historical insights. Barker (*British Policy in South-East Europe*), Deakin (*Embattled Mountain*) and Woodhouse (*Struggle for Greece, 1941-1949*) have recently published books broader in scope.

HARRY CLIADAKIS
University of South Carolina

ALUN CHALFONT. *Montgomery of Alamein*. New York: Atheneum. 1976. Pp. xiv, 365. \$12.95.

Lord Chalfont's controversial appraisal of Field Marshal Viscount Montgomery of El Alamein stirred a tempest in a teacup when it happened to appear in Britain in April 1976 just three weeks after Britain's greatest World War II general died. This storm was not at all surprising considering the nature of the subject and the author. Alun Chalfont was a young officer in the Second World War and, after a long career in the Army, retired to become defense correspondent of *The Times* and then minister of state for foreign affairs. Like many British politicians, he has a flair with words. Montgomery was a general in 1939 when Chalfont was a subaltern. He eventually was given command in the field after others had suffered a series of disasters. He succeeded by stressing the morale and training of his men and safeguarding their lives whenever possible, by singleness of purpose, and by winning victories. He emerged from the war as the most prominent, but not necessarily the most able, British general. Montgomery went on to CIGS and was then shifted sideways into the Deputy Supreme Commandership of NATO where he remained until 1958. After that, he cast himself in the role of elder statesman. Unfortunately, according to Chalfont, Montgomery was not only obsessive, he was unimaginative, lacked grace and tact, had an inability to get along with his superiors, and was often stupidly abrasive.

Recognizing that Ronald Lewin's *Montgomery as Military Commander* (1971) adequately covered generalship, Chalfont decided instead to see what made Monty tick. He interviewed him and his family, he read the records, and he drew his own conclusions. As a result, Chalfont is not as devastating as R. W. Thompson, but in a more elegant way he paints much the same picture. Monty was an absolutely dedicated professional soldier who had a clear idea of what should be done and who came onto the battlefield when war became a matter of modern technology more than individual bravery. Monty may have discovered this in the First World War, but was not able to put it into practice until the middle of the Second, when manpower was moving into short supply.

Much like MacArthur, Montgomery was and will remain a controversial character because of his various roles, because he was not the typical Army officer, and because he remained on the scene for thirty years after the war, picking quarrels or uttering banalities, as a result of that famous lack of tact or lack of understanding of the political milieu, or both.

Chalfont's book must not be tossed out. It is a reasoned account of Montgomery the man and the environment into which he was born, and in which he lived, worked, and died. There was an outcry

when Lord Moran published his medical biography of Churchill. Both that work and this one must be defended on the same grounds—their heroes are much too important to allow their own autobiographies to stand as their sole monuments.

ROBIN HIGHAM
Kansas State University

STANLEY L. FALK. *Seventy Days to Singapore*. New York: G. P. Putnam's Sons. 1975. Pp. 301. \$8.95.

There is a melancholy fascination in the contemplation of great disasters, a fact that historians have known since Thucydides, and one that filmmakers are now discovering. For a military historian few disasters are more spectacular, complete, or far-reaching than the British collapse in what was then known as the Far East. That military debacle culminated in the surrender, on February 15, 1942, of Singapore, an event that symbolized the ruin of the European order in Asia. The subsequent defeat of Japan and the bloody quarter-century of Western rear-guard actions that followed could not alter this simple and central fact. The tides of history had turned against the Europeans (and Americans) in Asia. The surrender of Singapore, like the fall of Constantinople in 1453, will become a pivotal date in history.

The two events have in common as well their military inevitability. Why defeat should have been unavoidable for the British is one of the important questions about the fall of Singapore that is unfortunately scantily treated in Stanley Falk's new, and in many ways excellent, study. He has carefully read all the published materials and produced a balanced, concise, and well-written account of the campaign itself. The official histories, which provide the framework of his account, were, however, rather thin on pre-1939 British defense policy and post-1939 War Cabinet priorities, and so is Falk. He also largely ignores the very controversial question of civil-military relations in Malaya, a topic on which the British official historians were not so much thin as misleading. The documentary basis for an examination of these issues has been available for some time. Most of the relevant British government documents have been open for quite a while, and the papers of those two ill-fated commanders, Lieutenant General A. E. Percival and Air Chief Marshal Sir Robert Brooke-Popham, are open. The civil side of the campaign can be studied in the Bryson collection at the Royal Commonwealth Society Library in London and in the papers of the much-maligned Governor of the Straits Settlements, Sir Shenton Thomas. Falk has not used any of these. His book

is very good campaign history, but we need to go far beyond that before we have a complete account of the fortress that never was.

RAYMOND CALLAHAN
University of Delaware

PETER GIBBON. *The Origins of Ulster Unionism: the Formation of Popular Protestant Politics and Ideology in Nineteenth-Century Ireland*. Manchester: Manchester University Press. 1975. Pp. vii, 163. \$15.00.

ROGER H. HULL. *The Irish Triangle: Conflict in Northern Ireland*. Princeton: Princeton University Press. 1976. Pp. ix, 312. \$15.00.

JOSEPH V. O'BRIEN. *William O'Brien and the Course of Irish Politics, 1881-1918*. Berkeley and Los Angeles: University of California Press. 1976. Pp. xiii, 273. \$14.50.

CHARLES TOWNSEND. *The British Campaign in Ireland, 1919-1921: The Development of Political and Military Policies*. (Oxford Historical Monographs.) New York: Oxford University Press. 1975. Pp. xiii, 242. \$22.00.

Though each of the four works under review seems to have been inspired, to some degree, by the current troubles in Northern Ireland, Peter Gibbon draws the most boldly revisionist lessons from the reassessments of Irish history catalyzed by those events. His book is the first substantial product of a new school of Marxist Irish historiography which rejects James Connolly's facile synthesis of Irish nationalist and working-class objectives, and views Ulster Unionism as a nationalism (or "quasi-nationalism") in its own right.

Gibbon traces the origins of three crucial components of Ulster Protestant ideology—Orangeism, conversionist revivalism, and a distinctive conception of "loyalty"—to the needs of particular Protestant socioeconomic groups created by the processes of industrialization and urbanization. He depicts both the Conservative and Liberal parties as dominated by the rising urban elite rather than the declining landlord class. Each party became patron of an important class of Protestant electors: the Conservatives of the Belfast working class, the Liberals of the large tenant farmers. The issue between them was uncertain, despite the defection of Catholics from the Liberals to the Home Rulers, until the 1884 Reform Act swamped the large tenant vote with their enemies, the small tenants and agricultural laborers.

Ulster Protestantism was thus politically fragmented in 1886—Gibbon goes so far as to imply that had the First Home Rule Bill been enacted no unified Ulster resistance like that of 1912-14 would have occurred. Between the First and Second Home Rule Bills, the urban entrepreneurs de-

feated the landed classes and symbolized their victory in the 1892 Ulster Unionist Convention, whose form implicitly conceded that the other three provinces (containing many gentry but few Protestant capitalists) might have to be written off. In the succeeding generation, Ulster Unionism became "a form of nationalism" in which urban Tories were able to maintain hegemony over the Protestant working class not as a result of conspiring with landlords to dupe them, but as a natural outcome of the social dynamics of Ulster history.

The documentation of many of Gibbon's points is thin and diffuse. Sometimes he cites whole manuscript collections, rather than particular documents. One often gets the feeling that he has decided, on the basis of sociological literature, what ought to be the case and then simply asserted that the facts fit the model. Nevertheless, the book deserves more serious attention than it has received in Ireland, where one reviewer has dismissed it with the inane comment that because Ulster Unionism involves emotional, racial, and irrational reactions, it "cannot be 'scientifically' dissected and analysed" (*Hibernia*, January 30, 1976). Gibbon has raised pertinent questions, and though his answers are often unsatisfactory, he has opened up a number of fascinating lines of inquiry.

William O'Brien was a skilled but eccentric agrarian agitator who married an heiress just before the Parnell divorce crisis split the Irish Party. Rather than assuming the leadership role he could undoubtedly have commanded within parliamentary nationalism, he used his wife's money to sustain for nearly three decades a gadfly role on the fringe of nationalist politics. His anti-grazier United Irish League did briefly give him real political influence around 1900. After land purchase was enacted in 1903, however, he set a quixotic course which confined that influence to his local electoral base in County Cork. Deluded by the friendly spirit in which the landlords had cooperated in formulating the scheme which would relieve them of an unprofitable patrimony, he sought to promote settlement of the Home Rule question by similar friendly negotiations with Protestants. Most Irish Protestants were not landlords, and O'Brien was unable to come up with a practicable alternative to the main lines of the Party's policy. He became a rather pathetic client of a few ex-landlords, and after 1918 he drifted into republicanism.

Joseph V. O'Brien's research in manuscript and printed sources is impressive—doubly impressive to anyone who has tried to decipher his namesake's scrawl. In general, he tells the story fairly, accurately, and readably. He manages to achieve more detachment from his idiosyncratic hero than from the traditions of nationalist historiography.

He writes that "there were but three main ways to solve the Irish question—exterminate the Unionist one-fourth of the population, exterminate the Nationalist three-fourths, or create conditions under which both sections could advance together in a shared independence" (p. 245). There was, of course, a fourth alternative—partition—but to admit that it was a "solution" would be unthinkable for the author, as it was for his subject. His historical scholarship is far sounder than Gibbon's, but his inability to think the unthinkable makes it harder for him to achieve new and fruitful insights.

Charles Townshend's analytic narrative of British policy in the Anglo-Irish War provides both sound scholarship and significant new insight. He has constructed from primary materials a fascinating story of divided counsels, uncertain objectives, and fatal divergences between intentions and outcomes, which adds greatly to our understanding of how policy was formulated on a day-to-day basis and of the creaky governmental machinery through which it was to be implemented. His evaluations of the practical and ethical implications of that policy are cautious, temperate, and consistent with the received wisdom of moderate British and Irish opinion. However, his work has a significant, perhaps understated, revisionist implication. His painstaking chronicle of government muddle and ineptitude gives us, for the first time, a useful corrective to the usual assumption that the outcome to the Anglo-Irish War was virtually fore-ordained either by the invincibility of a guerrilla force enjoying popular acquiescence or by the British public's low threshold of revulsion at official barbarity. This splendid monograph is required reading for all serious students of modern Irish history.

Roger Hull's *The Irish Triangle* is an attorney's attempt to elucidate legal aspects of the contemporary Northern Ireland conflict: the constitutional status of the province, applications of the law of civil strife and of war, problems of human rights, and a possible U.N. role. In each chapter the author provides a "noncontroversial" statement of facts and law, briefs for three "viewpoints"—Dublin, London, and Belfast—and his "personal" conclusions. His attempt to classify the positions of the participants under three "umbrellas" is not promising—especially as we learn in the introduction that "the Dublin viewpoint" is somehow broad enough to encompass the IRA, the Dublin government, and the Civil Rights Association. Actually, "the Dublin viewpoint" turns out to be approximately the view of the Fianna Fail Party. The author seems oblivious to the fact that that party lost the 1973 general election to a coalition with very different policies. An example of his failure to understand the situation is his suggestion

that "London" and "Belfast" concur in holding that "the IRA 'controls' no terrain." In fact, "Belfast" is seriously at odds with "London" on precisely this point; Loyalists frequently accuse the government of turning a blind eye to effective IRA hegemony over "no-go areas" in Belfast and Londonderry and in fairly extensive rural areas such as South Armagh.

The book does have some marginal value; Hull's summaries of strictly legal issues are occasionally instructive. On the whole, however, it is so historically naive that the only reason for noticing it in this journal is that its prestigious imprint might mislead the unwary into thinking it a work of significance.

DAVID W. MILLER
Carnegie-Mellon University

ARLETTE JOUANNA. *L'idée de race en France au XV^{ème} siècle et au début du XVII^{ème} siècle (1498–1614)*. In three volumes. Lille: Université Lille III; distributed by Librairie Honoré Champion, Paris. 1976. Pp. iii, 1,488. 150 fr.

Within the context of sixteenth-century French society, this book studies the idea that man's natural inequality is inherited and anchors the social hierarchy. Arlette Jouanna's method is four-fold: description and analysis of the idea of lineage; presentation of the collective mental structure of the time; sociological correlation of the lineage, social position, and mobility of authors to their particular versions of the idea; and chronological history. The extensive source base includes works of eighty-eight authors who expressed the idea, accounts of provincial and national meetings of the Estates, and popular literature. The book, a photoreproduction of a dissertation, is very successful in its clear exposition of attitudes within the context of social stratification. Jouanna's evidence that the idea of lineage was embedded in the popular outlook casts doubt, however, on her claim that the idea emerged only after 1550.

Jouanna's thesis is that the notion of noble superiority did not emerge as a partisan argument to defend the interests of the privileged (as argued in M. A. Devyver, *Le sang épuré. Les préjugés de race chez les gentilshommes français de l'Ancien Régime, 1560–1720*), but emerged as a spontaneous attempt by French nobles, lawyers, doctors, merchants, and commoners to relate their society to the natural order. Most authors identified noble virtue with valor, the characteristic of the traditional nobility of the sword. Among the nobility of the robe, only a few diverged from that viewpoint, usually to claim that prudence, exercised in court judgments, is equal in nobility to valor. Characteristic of the

new nobility and the bourgeoisie was the emphasis on the heredity of inferiority among social groups beneath them.

Between 1585 and 1595, the idea of lineage became controversial. Some jurists created a notion of societal position based on public utility and viewed prudence as being of the highest utility to the state; some members of the League rebuked the nobility for their irresponsibility during the wars of religion and defended the idea of mobility. The controversy culminated in the Estates General of 1614. The members of the Third Estate, mostly jurists and officers, urged the nobles to acquire skills of service to the state to justify their privileges. Therein emerged the defensive Second Estate claiming to have acquired their dignity through the blood and service of their ancestors from time immemorial.

MARYANNE C. HOROWITZ
Occidental College

ROGER CHARTIER *et al.* *L'éducation en France du XVI^e au XVIII^e siècle*. Paris: Société d'Édition d'Enseignement Supérieur. 1976. Pp. 304.

Under the influence of anthropologists, demographers, and sociologists, the history of education in recent years has moved far from its traditional preoccupations with educational policy, institutional development, and pedagogical change. The barriers between schools and society have fallen, and out of the rubble have emerged studies on enrollment statistics, the social and professional origins of students, patterns of postgraduate careers, and the social meaning of examinations and other pedagogical tools, each of which in its own way seeks to explain the complex and changing relationship between education and the outside world.

The present volume belongs to this genre, and its three young authors candidly admit that one of their goals is to bring the history of French education in line with "the history created and loved by the heirs of *Annales* or Ernest Labrousse." Undoubtedly this explains why nearly one quarter of the text is devoted to a somewhat baffling array of forty-three charts, six graphs, and sixteen maps, a number of which could easily have been omitted. In keeping with the penchant of *Annales* for interdisciplinary scholarship, inspiration for this work has come chiefly from sociology, especially Pierre Bourdieu's theory that education, rather than altering an existing social structure (providing means of social ascent), works to confirm or to "reproduce" it by reinforcing existing differences in status, culture, and wealth. This approach is not without value, but the authors, in attempting to make "reproduction" the leitmotif of their

study, occasionally forget that the statistical indices of social mobility used to support this hypothesis often obscure subtle changes in rank and status to which education in the Old Regime led. Consequently, the France they have depicted is rather more divided and rigid than it actually was.

The book is steeped in this sociological approach to education, and most of its nine chapters are concerned with the "function" of formal institutions of learning. Such a bias leads the authors to give only cursory treatment to apprenticeship and to ignore the family, which they admit remained the primary instrument of education throughout the period in question. Emphasis is upon those aspects of primary schools, colleges, and universities which can be quantified. Accordingly, most of the text is devoted to facts and figures on the location and distribution of schools, rates of male and female literacy, attendance at *collèges* and universities, although one chapter with a fresh discussion of pedagogical currents in the sixteenth and seventeenth centuries provides welcome relief in this statistical sea.

In sum, this volume, rich in information and detail, provides a comprehensive and up-to-date introduction to the social history of French education during the *ancien régime*.

RICHARD L. KAGAN
Johns Hopkins University

ROLAND MOUSNIER. *Recherches sur la stratification sociale à Paris aux XVII^e et XVIII^e siècles: L'échantillon de 1634, 1635, 1636*. (Publications de la Sorbonne, N. S. "Recherches," number 22. Travaux du Centre de Recherches sur la Civilisation de l'Europe Moderne, volume 18.) Paris: Éditions A. Pedone. 1976. Pp. 139.

This work represents the latest attempt of the distinguished historian of *ancien régime* institutions and social structure to verify his conception of seventeenth-century France as a "society of orders." Analyzing a "representative" sample of 1,003 Parisian marriage contracts and 386 inventories after death, Roland Mousnier identifies the *qualités* and (secondarily) professions which allied in marriage. He designates these intermarrying groups as the nine orders and their twenty-nine constituent *états* and arranges them hierarchically according to contemporaries' evaluations of their ascribed functions.

The argument, method, and shortcomings are familiar to those conversant with the controversy over popular uprisings. Mousnier argues, with the fervor of a convert, that concepts of social class (either Marxist or generic) are inapplicable to seventeenth-century France. The strata are endogamous, difficult to enter, and linked less through

social mobility than through fidelities. Furthermore, the distribution of fortunes reveals that neither the production nor the possession of wealth determines the hierarchy of prestige.

Yet, Mousnier may underestimate the structural consequences of female hypergamy, which calls into question the inference of social equivalence from marital pairings, and he may exaggerate the degree of consensus on the relative esteem due various functions. Indeed Mousnier's static grid cannot accommodate forces of change and conflict that his own examples display. The sample size is inadequate for the large number of categories.

Nonetheless, this is a study "*sans prétension statistique*" (p. 105) which analyzes social structure qualitatively because the nuances of status do not coincide with the numbers available (wealth) and can be expressed only in a statistically unmanageable number of categories. Because it offers abundant detail honed judiciously into a more discriminant classification (with unusually refined distinctions among the *métiers*) than has hitherto been available, it will serve as a handbook for researchers pursuing the questions of social structure Mousnier has raised.

CAROLYN C. LOUGEE
Stanford University

TIHOMIR J. MARKOVITCH. *Histoire des industries françaises: Les industries lainières de Colbert à la Révolution*. (Travaux de droit, d'économie, de sociologie et de sciences politiques, number 104.) Geneva: Librairie Droz. 1976. Pp. 501.

Tihomir J. Markovitch is writing a three-volume history of the French woolen industry from the age of Colbert to modern times. This book is the first installment and carries the story to 1789; it also inaugurates a new *Histoire des industries françaises*, to be published by the *Institut de science économique appliquée*.

As befits a member of the ISEA, the author employs a rigorously quantitative analysis and, unlike the *annalistes*, seldom explores the social implications of his statistical findings. His objective is rather to establish indices of production, particularly for the eighteenth century, and to measure the growth of French woollens at the national level and locally—in five broadly defined "regions" and in each of the kingdom's thirty-two *généralités*. Statistics for production are worked up not only by units (in pieces of cloth), but also by area (in the square *aune*) and in market values (*livres tournois*). This approach forces no radical changes in our understanding of the industry, but it fortifies current views and yields interesting new insights.

For example, the axiomatic hegemony of north-

ern France is abundantly reconfirmed: in 1789, as in 1715, this region produced almost fifty percent of French woollens, far outdistancing southern and central France, its primary rivals. Moreover, Markovitch makes clearer than ever the conversion in the major northern centers—Elbeuf/Louviers, Amiens, Abbeville, Reims, and Sedan—to large-scale production of the finer, more expensive fabrics and their success in domestic and foreign markets. This change also concentrated the industry in large, efficient enterprises and forced the decline of smaller manufacturers. In Sedan, indeed, there appeared "*véritables établissements industriels au sens plus moderne du mot*" (p. 151). To a considerable extent, especially at Carcassonne, similar changes occurred throughout the other woolen-producing areas, but on a vastly reduced scale.

All areas enjoyed growth in the 1700s, and the cycles which Markovitch establishes harmonize with the pioneering work of Simiand and Labrousse. With rare but important exceptions, as at Sedan, the annual growth rates were as moderate as Pierre Léon has suggested: less than one percent.

For all his merits, the author makes few concessions to persons who believe history to be a form of literature and it would seem that more of his statistical findings could best be presented in the form of tables or charts. Like the rest of us, moreover, he is obliged, when contemporary statistics are lacking, to resort to imagination, inference, and extrapolation from the known to the unknown. The sources themselves may be open to some question: they consist of official documents, particularly of reports from the inspectors of manufactures. But the author is aware of the problems these documents present and promises a full discussion of sources and statistical methods in a forthcoming volume. Meanwhile, his figures take precedence over those of his predecessors. His valuable book will be consulted by economic, social, and regional historians, who will wish to draw out the full implications of his statistics.

JOHN J. HURT
University of Delaware

ALAN CHARLES KORS. *D'Holbach's Coterie: An Enlightenment in Paris*. Princeton: Princeton University Press. 1976. Pp. xi, 359. \$17.50.

This is a myth-exploding book, much needed, meticulously researched, and convincingly argued. The myth states that Baron d'Holbach harbored the most rabid wing of the French Enlightenment in his salon, where militant atheists laid plans for disembowelling priests and strangling kings, and collaborated on works more harshly and dogmat-

ically intolerant toward believers than belief ever was toward doubt. Milder forms of the myth have been held, but they all support the same thesis: d'Holbach's coterie embodied the materialist cause.

Alan C. Kors builds a patient case against the myth, identifying the fifteen members of the coterie, establishing their individual convictions and activities, relating them to the social structure of the *ancien régime*, characterizing their discussions, and finally tracing the fate of the surviving members after 1789. What emerges illumines both the relationship between the Enlightenment and the *ancien régime*, and the relationship between the Enlightenment and the Revolution.

The three parts of this well-organized book make three points: 1.) The distinctive and unifying force of d'Holbach's coterie was not atheism, and certainly not a collaborative assault upon religion, but candor. In the rue Royale there were no taboos, no proprieties, as there were in other salons, to inhibit argument or speculation; here the philosophes could speak their convictions, whatever they were. 2.) Far from being a renegade group, the members of the coterie held favored positions in the *ancien régime*. They had achieved status, privilege, and sometimes wealth through the normal avenues of influence and patronage, securing election to academies, appointment to sinecures, nomination to benefices, and the like. 3.) Those who lived into the Revolution were appalled by its course after the summer of 1789 and risked much to say so.

It is clear from this study that the Enlightenment, even the atheistic Enlightenment, belonged to the *ancien régime* and not to the Revolution. The philosophes, even atheistic philosophes, were not only tolerated, they were rewarded. Kors has perhaps too much emphasized the *principled* rejection of the Revolution by the seven survivors, and not sufficiently stressed their *interested* rejection of it. On Kors' own showing these men had a big stake in the *ancien régime*. That regime might have been detestable, but it had been good to them, and its collapse spelled the end of place and influence and security, as well as the end of their hopes for an enlightened, but still orderly, world.

ISABEL F. KNIGHT
Pennsylvania State University

STEPHEN ELLENBURG. *Rousseau's Political Philosophy: An Interpretation from Within*. Ithaca: Cornell University Press. 1976. Pp. 335. \$15.00.

At the same time as it avoids direct confrontation with other recent critical approaches, this admittedly adulatory interpretation of Rousseau's political theory maintains that the intertwined ideas of

non-individualism and anarchism offer significant insight into the entire corpus. Dismissing historical psychoanalysis as demeaning Rousseau's contribution and holding that an appraisal based upon Rousseau's debt to ancient thinkers distorts the meaning of his essential egalitarianism, Stephen Ellenburg constructs his argument upon Rousseau's supposed rejection of the premises of liberalism—particularly the one that “posits the existence of a discrete individual either with private interests or with rights claimed independent of common life.” According to Ellenburg, Rousseau's citizen is a public or artificial person, “a socially dependent creature, deriving his identity from the cohesive, social involvement in which he originates and of which he remains an indivisible part.” He is integrated within a common, corporate life; no other human being rules over him.

Offering an interpretation based upon Rousseau's advocacy of literal self-government within the framework of a disciplined common life, Ellenburg pursues the themes of sincerity and simplicity as guides for virtuous citizenship and concludes with Rousseau's “vision of self-government” as worked out in *The Social Contract* and, more originally, in *Émile*. The argument is repetitive and although the interpretation is avowedly personal, one still wishes that Ellenburg would challenge Vaughan, Shklar, Talmon, and Crocker more directly than in occasional footnotes. For example, by placing the time of *The Social Contract* within the third era of the *Discourse on Inequality*, thus rendering the idea of social redemption impossible, Ellenburg not only converts the great revolutionary document into an admittedly “melancholic *histoire*,” but also implicitly challenges a well-entrenched scholarly tradition. Perhaps, as Ellenburg notes, “Rousseau was concerned with abstract principles of political right, not their ‘applications’.” But the point is insufficiently developed.

Though liberal critics will have difficulty with Ellenburg's cursory treatment of religious apostasy in *The Social Contract* and feminist critics will be hard put to reconcile the perceptive brief section on women with Ellenburg's fulsome praise of the Rousseauist idea of freedom, this clearly written essay deserves a place beside the other important interpretations of recent years—most notably those of Launay, Masters, Polin, Charvet, and Hall.

RAYMOND BIRN
University of Oregon

MICHEL VOVELLE. *Religion et révolution: La déchristianisation de l'an II*. (Le Temps et les Hommes.) Paris: Hachette. 1976. Pp. 316. 52 fr.

Michel Vovelle's interest in the transformation of eighteenth-century religious attitudes is already well known from his remarkable study, *Piété baroque et déchristianisation en Provence* (1973). His most recent book, on the de-Christianizing movement of the year II, is to be seen as both sequel and complement to his *thèse* and as a major contribution to a relatively neglected area of French Revolution studies.

As in his earlier work, Vovelle confines himself to southeastern France, though here a "South-east" more broadly conceived, extending from Provence through southern Burgundy and Nivernais. Much of the book consists of an empirical and highly quantified description of the geography, chronology, and outward manifestations of the de-Christianization, based almost entirely on two sets of sources: lists of clerical abdications of the priesthood and those reports, speeches, etc., sent from the provinces to the Convention and published in the *Archives parlementaires*. The author is thus able to identify a series of de-Christianizing waves initially emanating from Central France and continuing southward over a period of several months—analogueous to but far more complex than the Great Fear of 1789.

It is above all the geography of the movement, the enormous range of regional reactions, from rejection and rebellion to passive or even welcome acceptance, that leads Vovelle to the interpretive concluding chapters of the book. He explores the major role in provoking the movement played by such outside agents as the representatives on mission and the revolutionary armies. But he also notes the strong inverse correlation between the intensity of de-Christianization in the year II and the degree of regional religious vitality both after and before the Revolution—evidence, he believes, of another, long-developing "de-Christianization" in certain areas, revealed and brought into focus by the events of the Terror.

It is a provocative and highly original study that will not fail to incite criticism of both methodology and interpretation. To many readers, the author's foray into linguistic analysis will seem somewhat rudimentary (Vovelle himself describes it as experimental and tentative). One might also question the advisability of grouping together, in the analysis of the "*prêtres abdiquaires*," clergymen who had renounced the priesthood with those who had merely resigned their ecclesiastical functions. (Such a distinction has been found particularly revealing in recent studies of de-Christianization in Upper Dauphiné.) But if some of Vovelle's suggestions and conclusions must await confirmation by more intensive local studies, supported by a greater diversity of sources, his work is, nevertheless, a significant achievement that will give

new direction, and perhaps new interest, to research into this critical period in the evolution of popular religious culture.

TIMOTHY TACKETT
Marquette University

CHANTAL DE TOURTIER-BONAZZI, compiler. *Lafayette: Documents conservés en France*. Volume I, *Archives nationales—Service historique de l'armée, archives du ministère des affaires étrangères*. Foreword by JEAN FAVIER. Paris: Archives nationales. 1976. Pp. 380. 150 fr.

Prepared by the curator of the *Archives privées* in the *Archives nationales*, this catalogue is the first volume of a project to list and describe all manuscripts, letters, and documents relating to the Marquis de Lafayette located in French library and archival collections. Listed here are Lafayette materials held in official French archives, specifically in the *Archives nationales*, the *Service historique de l'armée*, and the *Archives des affaires étrangères*. The arrangement of the guide is by repository, with each section showing the materials as they are organized in the particular institution. Such archival arrangement means that the three indexes provided—geographic, correspondent, and partial subject—are both a helpful and very necessary part of the guide. A descriptive bibliography at the beginning of the volume is also most useful. The only lack would seem to be a chronological approach to the materials so that all documents relating to a specific period of time might be located more easily.

Most of the documents in the three archives relate to Lafayette's activities and public life in France after the American Revolution. This is understandable since the majority of papers concerning his American "career" is located in United States repositories. (A similar guide to Lafayette materials in United States collections was published by Cornell University in 1975). The descriptions provided in the catalogue are concise, yet detailed enough to let the reader know exactly what each item is. As a bibliographic tool this volume will certainly facilitate the identification and use of Lafayette documents, thereby assisting scholars in obtaining greater knowledge and understanding of a historic figure so important to two continents.

SAUNDRA TAYLOR
Lilly Library
Indiana University,
Bloomington

LINDA ORR. *Jules Michelet: Nature, History, and Language*. Ithaca: Cornell University Press. 1976. Pp. xvi, 215. \$12.50.

The lively renaissance of Michelet studies continues on both sides of the Atlantic—now fully three decades after Edmund Wilson and Lucien Febvre “rehabilitated” the historian. Linda Orr’s fascinating new study builds upon such important work of the 1950s as Roland Barthes’ bold explorations and Paul Viallaneix’s masterful orienting and editing contributions. Unlike her predecessors, however, Orr derives her interpretative constructs mainly from a close examination of the nature books: *L’oiseau* (1856), *L’insecte* (1857), *La mer* (1861), and *La montagne* (1868). These poetic studies, she shows, should not be regarded as the frivolous works of an ageing historian simply yielding to his young second wife’s interests; rather they were a continuation of, indeed an illumination of, the historian’s central concerns—with understanding continuity/change and with finding a language for that understanding.

Armed with insights from recent literary criticism and other disciplines, Orr expertly guides us through texts wrought in Michelet’s troubled search for vistas blocked in his historical researches. The Michelet that she highlights is not the familiar dramatic combatant for liberty nor the man of firm humanist faith, but an ambivalent, ever self-critical, gropingly creative Ulysses. In contrast to the “simple, direct” “*voie royale*,” as Michelet once described his historical career, his naturalist thought was a “*marche sinueuse*” into a slippery “world of mystery and shadow” where certainties turn into ambiguities, dialectical flux, and ceaseless metamorphoses. Emphasizing his affinities with Rimbaud, Orr focuses on what she calls Michelet’s “ideological breakdown as revealed through . . . [his] language searching for its own content” (p. 160, n. 10).

Orr’s own writing, appropriately, is not as straightforward and easy a discourse as most earlier studies were. It weaves together observations on Michelet’s personal “myths” and self-images, his rhetorical devices and style, his personal experiences (childhood memories, “strange sexuality”), and it draws comparisons with other *littérateurs* (from Rousseau to Proust). Orr’s commentary is at times densely allusive, at times poetically elusive; it is also unfailingly stimulating and suggestive.

Michelet’s rhetoric, she concludes, “will both exasperate us and lead us into the complexities where his real interest is to be found” (p. 205). Her essay may well have similar results. Treating the writing of history as the writing of fictions (sharing some premises of Hayden White’s *Metahistory*), her account of Michelet’s experiments with language leads us to a fresh appreciation of his poetic genius; it may also fructify current thinking about the potentialities and limits of language and historical

knowledge—problems that Michelet probed with an extraordinary inventiveness and imaginative power.

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LYNN M. CASE. *Edouard Thouvenel et la diplomatie du Second Empire*. Translated by GUILLAUME DE BERTIER DE SAUVIGNY. (Bibliothèque de la Revue d’Histoire Diplomatique.) Paris: Éditions A. Pedone. Pp. 458.

This is both a biography of a diplomat, Edouard Thouvenel (1818–66), and an analysis of French diplomacy from 1849 to 1866. Lynn M. Case’s vast knowledge of mid-nineteenth-century French diplomacy, reflected in his previous publications, is bolstered in this work by his extensive use of the official archives of eight countries, the private correspondence of twenty-eight statesmen in three continents, and his judicious use of published documents and secondary studies. The most telling source is Thouvenel’s private papers.

Thouvenel served three governments during his diplomatic career and was often in sensitive posts at moments of crisis: in Athens he moderated Palmerston’s exorbitant demands during the Don Pacifico Affair; when he was political director (undersecretary) at the Quai d’Orsay his skillful pen brilliantly defined the French Crimean War position; as ambassador to Turkey he dealt successfully with Stratford’s schemes in Constantinople concerning the autonomy of Wallachia and Moldavia and the rights of the Sultan’s Christian subjects as defined in the Paris peace settlement of 1856; as foreign minister (1860–62) he negotiated the unification of Italy, the Syrian expedition, and the early problems of the Mexican intervention and the American Civil War. Perhaps his most lasting contributions were his articulations of the guidelines for the 1864 September Convention which established Italian-Papal relations as confirmed in the 1928 Lateran Accord, his note on the Trent Affair (1861), which was instrumental in persuading the North to yield Slidell and Mason to the British, and his special arbitration of the Suez Canal dispute (1864). Throughout, Case focuses not so much on the issues (already well known) as on the man and his influence on those issues. Ironically, although Thouvenel was naturally inclined to be pro-British, his struggles against English policy during the Don Pacifico Affair, in Constantinople, and over French annexation of Savoy and Nice resulted in a growing estrangement between London and Paris.

By using the personal correspondence of Thouvenel and his influential friends, Case casts

light on the relationships among the various leaders of the Second Empire, thus revealing Napoleon III's problems and abilities in balancing and checking diverse subsurface political and social forces. From this emerges a clearer image of the emperor, with a broader view than any one of his ministers, delicately handling simultaneously the internal and foreign problems of France.

The author's scholarship and the translator's fluid French make this book worthy of Lynn Case, already recognized as the dean of nineteenth-century French diplomatic history. One does not have to agree with his evaluation of Thouvenel as "the best" of all Napoleon III's foreign ministers to accept this study as a definitive, positive contribution to the field of nineteenth-century diplomacy.

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EUGEN WEBER. *Peasants into Frenchmen: The Modernization of Rural France, 1870-1914*. Stanford: Stanford University Press. 1976. Pp. xv, 615. \$20.00.

In this massive synthesis, based on hundreds of regional studies and contemporary accounts, Eugen Weber argues that France became truly a nation only in the late nineteenth century when her poorer rural regions of the west, center, south, and southwest developed a national consciousness. Distinguishing between an administrative unity and a "moral, mental, and cultural" one, Weber traces the acculturation process in the countryside which transformed peasants into patriots. The unraveling of the process evokes the material and cultural life of nineteenth-century rural society with a richness of detail previously unavailable in English.

Though his many examples are enlivened with wit and proverbs, Weber, nonetheless, creates a somber image of the countryside before the 1870s. Material life is characterized as one of unrelieved misery. Poor diet, unhealthy lodgings, lack of roads, overabundance of crime, economic isolation, and limited agrarian knowledge all, Weber concludes, served to keep peasants passive. Of more significance were the cultural differences between the countryside and the rest of France. Local dialects insulated regions; each celebrated its own festivals, believed in its saints and witches, developed its legends and songs, and understood little of political life. Language diversity, then, was the key obstacle to rural society's development, because it slowed down the diffusion of ideas from the city to the countryside.

Change disrupted rural society in the early years of the Third Republic as the welter of urban forces did spread throughout the countryside. First,

roads and railways altered material life, then schools and the army funnelled urban values, among them patriotism, to peasants. Brilliantly, Weber shows how schools and the army only provided a common experience for the countryside late in the century, after new laws, such as the elimination of school fees (1867) and universal conscription (1889) made these institutions effective. Increasingly exposed to French at markets alongside railway lines, forced to listen to French spoken by army officers, taught French in the schools, peasants finally learned the official language and through it acquired patriotic feelings.

The study's major weakness is its rigid adherence to the rural-urban dichotomy, according to which rural society, backward and retrograde, crumbles under the impact of urban values. A more complex image of the countryside emerges from studies where village conflicts are analyzed to reveal the internal pressures and options for social and political change. Weber dismisses this evidence without scrutiny. Likewise the distinction between rural-urban attitudes breaks down in the case of some factory workers who did not exhibit "modern" traits until late in the century. Both the rural conflicts and the experience of factory workers underscore the importance of local centers of modernization, whether urban or rural. Too facile an acceptance of the rural-urban dichotomy prevents Weber from constructing a more complex interpretation of rural society.

The value of this study is immense, sure to spark fruitful debate among specialists and generalists alike: it is the first major synthesis of nineteenth-century rural society and it illuminates the making of the French nation. A significant question to be raised is whether peasants became Frenchmen or, like other Frenchmen, became patriots late in the century.

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WILLIAM I. SHORROCK. *French Imperialism in the Middle East: The Failure of Policy in Syria and Lebanon, 1900-1914*. Madison: University of Wisconsin Press. 1976. Pp. x, 214. \$17.50.

William I. Shorrock has written a work which attempts to identify the origins of French involvement in what would eventually become the republics of Syria and Lebanon and by so doing to explain why this involvement eventually failed in the mandatory period. Shorrock proposes that by understanding the failure of French policy we may also gain insight into the growth of Arab nationalism.

In elaborating his thesis the author, in a pains-

taking effort, identifies three major areas of French penetration into Lebanon and Syria: religious, political, and most important, economic. In this last area, the French believed that when the Ottoman Empire died, they could best claim some of the empire's territory if they first staked out an economic zone in part of it. To this end, though they at first championed Ottoman reform in geographic Syria, they abandoned this policy in 1913 and 1914 in favor of their own economic interests in the area. This, the author argues, led to disillusionment among the Syrian people and in large part explains Syrian resistance to, and the ultimate failure of, the French mandate.

Though the book is well-written, I believe the author has not proved his thesis—probably because it is one impossible to prove. To argue that the espousal of economic interest doomed the French mandate seems tenuous at best. Implicit in this argument is the notion that if the French had continued to stand with the Arabs in the latter's call for Ottoman reform, then the French mandate would have been welcomed and successful. This seems unlikely. The French mandate ultimately failed because it was foreign and imperialistic.

Where the book has real merit is in showing us how the European political system worked in the late nineteenth and early twentieth centuries. Shorrock gives us excellent insight into the minds and motivations of the various Frenchmen charged with formulating and carrying out policy in the Middle East. But he tells us virtually nothing about what the recipients of these policies were saying. Not one Arabic source is used. Still the author writes, "And the documents show conclusively that after the publication of the Franco-Turk [economic] accord in April 1914, Syrian opinion turned radically and decisively against French influence in Syria" (pp. 166–67). The author might argue that European documents suffice to prove his statement, but his argument would have been more forceful had he perused the Syrian archives.

The book, then, tells us a great deal about France but not much about Franco-Syrian relations. I had hoped that the day was gone when a historian would write about European diplomacy without using the sources of those who received the attention of Europe. By neglecting the "natives," we are like the blind man who holds only the elephant's tail and concludes it is a snake.

WILLIAM W. HADDAD
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STEPHEN A. SCHUKER. *The End of French Predominance in Europe: The Financial Crisis of 1924 and the Adoption of the Dawes Plan*. Chapel Hill: University of North Carolina Press. 1976. Pp. xv, 444. \$20.95.

This is a superb study of the climactic—or perhaps anticlimactic—phase of the reparation conflict, the Dawes Plan negotiations of 1924. Unlike many historians, Stephen A. Schuker quite properly stresses the intense political significance of this conflict: the real question was whether the Versailles system would remain intact or whether German power would again be allowed to dominate Europe. Schuker demonstrates that the events of 1924 marked the defeat of those who wished to uphold Versailles. Henceforth France was committed to a policy of conciliation; the use of coercion against Germany was virtually ruled out; the resurgence of an "inherently expansionist" Germany as the dominant power on the continent became more or less inevitable.

For the historian, the fundamental problem is to explain why events took this course. Given Schuker's view that France, in the immediate postwar period, was the only major power "committed to an integral defense" of the Versailles system, this boils down to a question of French policy: why did France fail? Schuker stresses internal factors. France was unnecessarily vulnerable because her leaders had mismanaged public finance. This in turn was a reflection of certain deeper defects in her political system and political culture: the "governing class" was "abysmally ignorant" of economics; the bureaucracy was often "paralyzed" by "internecine rivalry"; the parliamentary structure was an anachronism, ill adapted to the industrial age; "petty considerations of domestic politics" overshadowed larger diplomatic considerations.

There is little doubt that these were real problems, and scholars have long been aware of them. What distinguishes Schuker's study is the way he gives substance and life to these somewhat abstract notions: the detail is wonderfully rich; the range of sources is absolutely unparalleled. Yet even conceding that Schuker's analysis is substantially correct, does this solve the problem? Focusing on internal factors implies that if only things had been different—essentially if France had been more modern—then the story might have developed along basically different lines. Indeed Schuker's insistence on the importance of his subject—the London Conference of 1924, he says, "marked a decisive turning point," "radically" altering the structure of European politics—implies that other outcomes were possible.

Schuker, however, is not perfectly consistent here. For at other times he implies that from the outset, given the attitude of Great Britain and the United States, the containment of Germany was just not in the cards. France alone was "no match" for Germany; Herriot's incompetence in diplomacy made only a marginal difference—on essentials Poincaré would not have done much better.

If there is an inconsistency here, it is one that should not be condemned too readily. Anyone who studies these questions in any depth finds himself drawn to both sides of this issue. But conflicting feelings of this sort were certainly even more powerful in the early 1920s. It was unclear from the beginning whether in the long run the Versailles system could be made to work. This, it seems to me, was the real root of the confusion, the lack of direction so characteristic of French policy in the period.

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JEAN-NOËL JEANNENEY. *François de Wendel en République: L'argent et le pouvoir, 1914-1940.* (L'univers historique.) Paris: Éditions du Seuil. 1976. Pp. 669.

Jean-Noël Jeanneney's purpose is to dispel the "myths" surrounding the name of François de Wendel, director of the famous metallurgical firm bearing his family name, president of the Comité des Forges, a regent of the Bank of France, deputy, then senator, from 1914 until 1940. De Wendel was seen by his critics, most of them on the left, as wielding an almost occult power over French political and economic life which he used ruthlessly to advance the interest of his family at the expense of the rest of the nation. Based largely on the voluminous journals kept by de Wendel and on other unpublished sources, Jeanneney's work focuses on the "public man." He tells us little of de Wendel's personal life or of the giant firm his family controlled.

For Jeanneney, the consideration dominating de Wendel's public life was patriotism. A desire to undo 1870-71 led him into politics in 1914, and his passion for French security and prestige never diminished. The author declares de Wendel innocent of the charge that he intervened to prevent bombardment of family holdings in Briey in World War I. He argues that patriotism, not personal gain, led de Wendel to support Poincaré's Ruhr policy and, later, to oppose the policies of Briand. Fears of retaliation against family properties in Germany moderated his anti-German utterances in the 1930s, but, unlike others on the right, he did not prefer Hitler to Stalin and, albeit with distaste, supported the Franco-Soviet defense treaty. He supported the attempts by Léon Blum to form a government of national unity in 1938 and, after 1940, refused to associate himself with Vichy.

Jeanneney also holds that de Wendel's influence in French public life has been greatly exaggerated. He acknowledges the important role of de Wendel in overturning the *Cartel des Gauches* in 1926, but insists that blunders by *Cartel* leaders also figured

largely in the outcome. Moreover, there was no simple "refusal" by a *mur d'argent*, for on this, as on other occasions, French business interests were divided in their attitudes toward the government. Additionally, if de Wendel "won" this one, he usually was unsuccessful in his attempts to shape government policy; most notably in 1928 when, contrary to de Wendel's desire for continued *revalorization* of the franc, Poincaré chose stabilization. Jeanneney uses this episode to demonstrate that divisions within the French business community often gave political leaders like Poincaré more freedom to maneuver than some observers have allowed.

This important study does much to rehabilitate de Wendel's reputation and to put the connections between French business and government in these years in more accurate perspective. But Jeanneney is not completely successful, for, as he admits, de Wendel's political and social views coincided closely with the interest of the possessing classes to which he belonged. He may not have been the villain his critics on the left claimed, but they did not err on the fundamental point.

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LOUIS-ANDRÉ VIGNERAS. *The Discovery of South America and the Andalusian Voyages.* Chicago: University of Chicago Press. 1976. Pp. ix, 170. \$10.00.

The role of early Spanish voyages in the delineation of the South American coast has long been a subject of historical controversy among scholars of Iberian maritime history. In his latest work Louis-André Vigneras offers so much information about these obscure voyages that, though one may disagree with his interpretations, his account remains intriguing. The author divides his narrative between eleven Andalusian voyages to the South American coast before 1506. Drawing upon a wealth of Spanish archival records, including legal and notarial accounts, Vigneras clearly outlines the pre-eminent economic motivations underpinning the Andalusian sailings. Each expedition required complex planning; the details of legal and financial commitments by each expedition commander is brought into sharp focus. No matter how important the discovery of new lands might have been to the Catholic monarchs, a horde of creditors awaited each returning commander.

An appendix details the historical contradictions surrounding Vespucci's checkered career in the American voyages, first in the company of Andalusian sailors and later (when foreigners were excluded from Spanish sailings) in the service of Portugal. The author's apparent confirmation of

Vespucci's claim of reaching Patagonia during his 1501-02 voyage is certain to inject more heat into the debate over the Florentine's true role in the discovery of South America's eastern shore.

Occasionally the reader is bombarded by such a superfluity of facts, particularly names of minor participants in the various expeditions, that the voyages themselves become secondary to the minutiae. Scholars in related areas of the discoveries doubtless will find useful cross-references in these names for their own research. Despite a sometimes cluttered narrative, this book is a valuable and much needed presentation of Europe's first glimpse of South American lands.

JOHN VOGT
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A. GORDON KINDER. *Casiodoro de Reina: Spanish Reformer of the Sixteenth Century*. London: Tamesis Books. 1975. Pp. xvii, 142. £10.50.

This is a well-documented and thoroughly researched book, but it is so compressed in details that sometimes it is rather slightly developed. Even so, A. Gordon Kinder's monographic study on Casiodoro de Reina (ca. 1520-94) gives us a complete picture of the man and his times. Kinder's sympathetic study allows us to penetrate the intrigue and hatred which followed this gentle Spaniard in his many flights from Protestant enemies and the Spanish Inquisition.

In the introduction Kinder presents an interpretive survey of the Spanish religious background from which Reina emerged as a monk from the Observatine Hieronymite Order of San Isidoro del Campo, in Seville, a reputed focus of Spanish Protestantism. Relying on basic works, Kinder sometimes seems to accept as conclusions views which are still debatable. One case in point is with respect to Constantino Ponce's death in the Inquisition's cell which Kinder says was "possibly by suicide." Another is the question of Luther's influence in Spain as early as 1519. It is one thing to have letters of Johann Frobenius indicating that he sent books to Spain; it is another thing to conclude that those books were smuggled into Spain, avoiding the Inquisition. The last point is with regard to Juan de Valdés, of whom Kinder says "he was still officially a member of the Roman Catholic church" at his death. What was "officially" true for Valdés, as for the Nicodemites, is not what is important but rather what was true in his *private* life.

The main chapters of the book, devoted to the biographic study, are excellent and offer new data and information on areas previously rather vague or unknown. It is unfortunate that Kinder provides only scanty information about Reina's most en-

during contribution, his translation of the Bible into Spanish, and pays little attention to Reina's religious ideas. This could have been partially remedied by including in the appendix the texts of Reina's two confessions of faith, one Calvinist and the other Lutheran! These *Confessions* are little known and as yet unpublished; besides, they are historically very important as the only extant confessions from Spanish Protestant refugee churches abroad. Publishing costs may have prohibited their inclusion. Historians of sixteenth-century Spain and of the Reformation will find this study very useful.

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R. L. SCHUURSMA. *Het onaannemelijk tractaat: Het verdrag met België van 3 april 1925 in de Nederlandse publieke opinie*. [The Unacceptable Agreement: The Treaty with Belgium of April 3, 1925 in Dutch Public Opinion]. Summary in English. (Historische Studies, number 31.) Groningen: H. D. Tjeenk Willink. 1975. Pp. x, 360. f 48.00.

The principal sources for this detailed public-opinion study are a large number of leading Dutch newspapers and materials relating to the so-called National Committee, one of the leading spokesmen of the opposition, and the Great Netherlands. The author first gives a sketch of Netherlands-Belgian relations since the treaty of 1839. By this treaty the Dutch retained so-called Zeeland Flanders, a small piece of territory south of the Western Scheldt, and Limburg. The Dutch granted free access to Antwerp through the Western Scheldt estuary but Belgian vessels were to be excluded from it in time of war. After World War I Belgium demanded the annexation of Limburg and Zeeland Flanders. Britain and the United States, however, opposed Belgian annexation plans, and the negotiations came to nought.

R. L. Schuursma then traces the negotiations, which were resumed in 1924, that culminated in the signing of a treaty in April 1925. This time it was Dutch Foreign Minister H. A. Karnebeek who initiated the talks, but why he decided upon this course is still not very clear to me. The treaty provided for free traffic through, and joint maintenance of, the Western Scheldt. Belgian war vessels were to be barred from this estuary, but subsequent explanations seemed to cast doubt on that provision. Belgian pilots would also be used in the so-called Wielingen channel, one of the principal Dutch entrances to the Western Scheldt. In addition, the treaty projected the construction of a canal from Antwerp to Moerdijk (near Dordrecht) and a Scheldt-Rhine canal through Limburg.

Dutch public reaction to the treaty was very

negative, and it was especially Anton Mussert, the future leader of the Dutch National Socialist Movement, who played an important role in the National Committee. The author makes it very clear, however, that opposition to the treaty was never a fascist affair. The Great Netherlands, such as historian Pieter Geyl, opposed the treaty because they feared it would enhance French influence at the expense of the Flemish. Since Dutch political parties were divided over the issue, Karnebeek failed to secure a majority.

Few issues stirred the Dutch more in the 1920s than this treaty, and this study provides us with a most interesting and important aspect of their political life. Who would have expected the internationally minded Dutch to be so chauvinistic and nationalistic and so eager to defend their national self-interest? The repercussions of the rejection of this treaty were to last until the 1960s; it was not until 1963 that a new treaty, providing for an Antwerp-Rhine canal, was finally concluded.

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N. K. C. A. IN 't VELD, editor. *De SS en Nederland: Documenten uit SS-Archieven, 1935-1945*. Volume 1. *Inleiding/Documenten, 1935-1942*; volume 2, *Documenten, 1943-1945*. (Ministerie van Onderwijs en Wetenschappen. Rijksinstituut voor Oorlogsdocumentatie. Bronnenpublicaties, Documenten, number 2.) 's-Gravenhage: Martinus Nijhoff, 1976. Pp. x, 908; 909-1714.

Under the title of "The SS and the Netherlands" 656 documents of varying length and importance are here reprinted in the original German. The extensive editorial footnotes are in Dutch, as is the 424-page commentary (labelled an introduction!) by N.K.C.A. in 't Veld. The non-Dutch reader will find a twenty-three-page summary of in 't Veld's comments in English in volume two, as well as a very brief English description of each document. Also added are twenty-seven photographs in volume one, a glossary of titles, and tables of SS ranks and organization in volume two.

The time span indicated in the title is somewhat misleading; only fourteen documents cover the years 1935-39, the rest are for 1940-45. These sources are from the Himmler files at the Berlin Document Center where they were selected from the *Hauptamt Persönlicher Stab Reichsführer-SS*, that is Himmler's own office files. Reference to these documents can also be found in volumes thirty-two, thirty-three, and thirty-nine of the *Guides to German Records* microfilmed at Alexandria, Virginia.

The purpose of this collection, as set forth in chapters one and five of the commentary, is to

discuss the political role of the SS in the Netherlands. Thus material on terror and police activities, persecution and deportation of the Jewish community, and the struggle with the underground is deliberately excluded. The political mission of the SS was the nazification of the Netherlands, and thereby the creation of a reliable satellite on the border of the Reich. From the German point of view the Dutch were eminently suitable by race, language, and history for such a process. But the Netherlands failed to respond to this historical opportunity. Even efforts to veil Nazi ambitions behind a screen of Greater-Germanic propaganda, in which supposedly the Dutch were co-equal with the Germans, had no success with the Dutch population.

After the initial failure to open a broad front of collaboration with the Dutch people, the SS turned for support to the Dutch Nazi Party, the *Nationaal Sociaalistische Beweging* or NSB. This effort would have no more success because the NSB had no mass basis and was regarded with contempt by the overwhelming majority of the Dutch. As a party it was also too middle class to serve the dynamic aims of the SS movement.

Ultimately the SS turned to recruiting Dutch volunteers for its own organization, in the hope that the indoctrinated Dutch SS would serve in the next generation as the pioneers for converting the general population to Nazi racial ideals. Exact figures are not available, but it is estimated that between 22,000 and 25,000 Dutch volunteers joined the *Waffen-SS*. The demand for service on the eastern front and the end of the war eliminated any chance of acting as an indoctrination corps, but in 't Veld admits that a watered-down version of SS ideals was adopted by even the most apolitical volunteers.

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THOMAS CHR. WYLLER. *Christian Michelsen: Politiker*. Oslo: Dreyers Forlag, 1975. Pp. 250.

Although Thomas Wyller declares in his preface that Christian Michelsen has been dead for fifty years, and that it is time someone wrote his biography, this study is not biographical. Except for a brief sketch of his early life, it is restricted almost entirely to Michelsen as politician, with the aim of presenting his political profile. The author's focus is furthermore limited strictly to his subject. Broader details concerning society and politics are omitted. The author assumes that his readers are familiar with Michelsen's career in particular, and with Norwegian political history in general.

As would be expected, the study concentrates

more on 1905 than on any other period. The profile which Wyller presents is nevertheless complete in that it covers all phases of Michelsen's political activity. Wyller admits that he does not present any significant new details. As a political scientist, he is mainly interested in interpreting his subject from a number of different angles. He succeeds admirably. The book's major value lies in the author's wide variety of interpretation, providing both analysis of specific periods of Michelsen's involvement in politics and an overall assessment. While contrary interpretations are possible regarding some questions, as for example Michelsen's degree of commitment to parliamentary government, on the whole Wyller provides a convincing portrayal. The author is sympathetic to his subject, but by no means uncritical, despite Michelsen's legendary position as "*landsfader*." In brief, Wyller shows that Michelsen enjoyed his great triumph in 1905 thanks to the unique political situation as well as to his skill and charisma, but that he could not adjust to ordinary partisan politics after 1905 and failed in his bid to change prevailing conditions. Following his retirement, he could at times exert considerable influence, but his ambivalent attitude toward politics prevented him from direct and sustained involvement.

When researching this study, the author benefited considerably from having access to the late Jacob Worm-Müller's incomplete manuscript and archival collection. Within the limits that he has established, Wyller's study is of value in providing an overall perspective of Christian Michelsen as politician. Michelsen's full biography, however, still remains to be written.

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JAMES CAVALLIE. *Från fred till krig: De finansiella problemen kring krigsutbrottet år 1700*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 68.) Summary in German. Uppsala: Universitetsbiblioteket Uppsala. 1975. Pp. 319.

This study, emanating from Sven A. Nilsson's Uppsala seminar "Social and State Financial Problems in Seventeenth-Century Swedish Society," deals with financing Sweden's mobilization for the Great Northern War—from mid-1699 to mid-1700—before Charles XII carried the war onto enemy territory and made it pay for itself. In particular, the author seeks to evaluate the effectiveness of Charles XI's reforms after 1680, establishing a standing territorial army and an orderly system of state finance.

James Cavallie provides the first detailed, archival study of his subject. In 1921 Eli F. Heckscher argued that Charles XI's fiscal structure was so

rigid and so dependent upon taxation *in natura* that it compromised Sweden's warmaking capacity. Fredrik Lagerroth countered in 1928 that it was so loose that the nonmilitary budget broke down under the pressure of military demands. Neither viewpoint was, however, based on much research.

Cavallie analyzes Sweden's system for covering regular budgetary and extra-budgetary needs, sources and forms of revenue from the kingdom proper (Sweden and Finland) and from the German and Baltic provinces, accumulated war chest, problems of credit at home and abroad, and financial administration. He discusses in detail the particular military needs of 1699–1700 and the various regular procedures or special expedients through which they were met. Of particular interest are the ways in which financial imperatives led to a temporary loosening of royal absolutism and reassertion of the positions of the royal council, financial bureaucracy, nobility, and local estates in the trans-Baltic provinces.

Yet the system held. "The state had during the immediately preceding decades been systematically prepared for war" (p. 16). Cavallie emphasizes the contrast to Charles X Gustav's makeshift mobilization against Poland in 1655, covered by Hans Landberg in the same series (1969). He concludes, contrary to Heckscher, that revenues and reserves, mainly in specie, proved sufficient for the mobilization; contrary to Lagerroth he shows that it was financed outside the regular, standing budget, which was not thereby disrupted. Although Charles XII had no understanding of finance, his financial bureaucracy proved equal to the challenge. Following Charles XI's reforms, "the financing of the mobilization could now be carried out within the framework of a centrally directed system of appropriations, with improvisations but without confusion" (p. 275). Its effectiveness was soon proven in the Zealand and Livonian campaigns.

Cavallie has provided an admirable case study of the late seventeenth-century transition to standing armies and systematic *Kameralwissenschaft*, based upon Sweden's remarkably complete financial archives. Instructive parallels could be drawn, for instance, with Sweden's rivals—Denmark, Saxony, Brandenburg-Prussia, and later Petrine Russia; such comparisons would indeed have added further interest. There is a good, nineteen-page German *Zusammenfassung*.

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WILLIAM R. COPELAND. *The Uneasy Alliance: Collaboration between the Finnish Opposition and the Russian Underground, 1899–1904*. (The Finnish Academy of

Sciences and Letters, number 179). Helsinki: Suomalainen Tiedekatemia. 1973. Pp. 224.

The elusive world of underground organizers and organizations seeking to undermine the power and unity of Tsarist Russia provides much of the topic of William R. Copeland's investigation. The title of his study, however, does not fully reveal the emphasis of the work; Copeland clearly concentrates his investigation on the Finnish side of the collaboration and has much less to say of the Russian resistance. The adversary of the would-be collaborators, the Tsarist regime, the third potential party in what could have amounted to a triangle drama, is discussed even less. Thus Copeland concentrates upon and emphasizes the same party in a complex conflict situation which has traditionally been the primary object of Finnish students of that conflict. Nevertheless he has succeeded in shedding new light and in sharpening various nuances of the picture.

Copeland properly notes that the leadership of the Finnish resistance to Russification was largely in the hands of people who belonged to the higher social strata of their country and had Swedish as their mother tongue. One can, though, question his conclusion that this situation "impeded the Finnish labor movement's participation in the resistance struggle." Obviously labor could have set up a parallel organization if it had so chosen and desired. At the same time Copeland treads on firmer ground when he stresses the unwillingness of the Finnish bourgeoisie to become entangled with and to establish an alliance with the Russian revolutionaries. Konni Zilliacus, the active leader of the relatively small Activist section of the Finnish resistance, ran into serious problems with his countrymen when trying to bring about such collaboration. The moderate Finnish resisters dealt with respectable liberal Russians rather than with revolutionary Russians of the underground. Their aims were more limited as were those of the Russian liberals. In contrast, the vision of Zilliacus was broader. He was willing to try to unite practically any and every group in order to bring down the Tsarist system. To the Russian opponents of Tsarism, contacts with the Finns facilitated the transportation of illegal publications and gave them some lessons in dealing with the nationality question of the Russian empire.

Copeland's scholarship is careful, as can be expected from a Finnish dissertation. He also introduces Finnish material previously little known in the English-speaking world and makes a contribution to our knowledge of the relation of the nationality problem of the Tsarist empire to the Russian revolutionary movement.

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HERMANN WIESFLECKER. *Kaiser Maximilian I.: Das Reich, Österreich und Europa an der Wende zur Neuzeit. Volume 2, Reichsreform und Kaiserpolitik, 1493-1500: Entmachtung des Königs im Reich und in Europa.* Munich: R. Oldenbourg Verlag. 1975. Pp. xvii, 574.

Historians concerned with the reign of Emperor Maximilian I have usually stressed the incongruities in his actions and character. They have generally agreed that he was an amiable, intelligent, and vigorous man who displayed remarkable courage as an athlete and warrior, prophetic vision as a political leader, and outstanding talent as a scholar and patron of the arts. But Maximilian's biographers have also insisted that he was an impulsive dreamer, unrealistic in his judgments, rash in his commitments, uncertain of his objectives, careless and inconsistent in the implementation of his ideas. Handicapped by his own shortcomings and compelled to tackle complex problems with scanty resources, Maximilian endured many frustrations and defeats along with his noteworthy accomplishments. Though unsuccessful in rejuvenating the archaic edifice of the Holy Roman Empire, he did contribute to the consolidation of his Austrian lands by erecting an efficient, centralized bureaucracy on the Burgundian model and managed to lay the foundations for Habsburg pre-eminence in the sixteenth century with his policy of dynastic marriage alliances.

Hermann Wiesflecker has devoted more than twenty years to a reassessment of Maximilian's career. Convinced that existing scholarship has been shaped by "one-sided judgments" of the emperor and his deeds, Wiesflecker has exhaustively reviewed the secondary literature and done extensive research in published document collections as well as hitherto unexploited archival materials to write what will surely be a definitive treatment of his subject. The book under review is only the second of a projected five-volume set. More ambitious than a conventional biography, it treats (somewhat repetitiously) the momentous events of German and European history during the years just preceding the Diet of Augsburg in 1500 from the perspective of Maximilian's life. Wiesflecker has quite correctly insisted that only by incorporating every relevant theme into a genuine synthesis that approaches each issue as part of an interrelated whole can Maximilian's imperial policies and dynastic ambitions be evaluated properly.

Applying a traditional methodology to a conventional topic, Wiesflecker has added relatively little to our basic understanding of the late fifteenth century. His painstaking research has, however, yielded some provocative conclusions about the prolonged struggles over imperial reform. Recognizing Maximilian's personal weaknesses, Wiesflecker nonetheless argues that this energetic

Habsburg seriously attempted to revive the medieval imperial ideal, not merely to serve family interests or augment the power of his hereditary domains, but to enable the Empire to play what he regarded as its proper leadership role in the emerging state-system of continental Europe. Wiesflecker places primary responsibility for the failure to solve pressing German problems on the territorial princes, who feared strong centralized authority and for whom the old concept of a universal empire had little meaning. While American readers will hardly be surprised by contentions such as these, they should welcome the opportunity provided by Wiesflecker to re-evaluate the reign of Maximilian I.

JOHN A. MEARS
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WERNER ZEISSNER. *Altkirchliche Kräfte in Bamberg unter Bischof Weigand von Redwitz (1522-1556)*. Bamberg: Historischer Verein für die Pflege der Geschichte des ehemaligen Fürstbistums Bamberg. 1975. Pp. xviii, 346. DM 28.

The principal concern of this dissertation, completed at the University of Würzburg in 1974, is to explain how the old Church was able to hold on in the Franconian Prince-Bishopric of Bamberg from the outbreak of the Reformation until the triumph of the Counter Reformation at the end of the century. Thus Werner Zeissner does not focus on the spread of the Reformation there, as previous literature generally has done, but on the "*altkirchliche Kräfte*" that opposed it, namely the bishop, the chapter, other ecclesiastical officials, and the local mendicants. He concludes that they deserve some credit just for preserving Catholicism during Bishop Weigand's reign, even though he agrees with the assessment of the nuncio in 1573 that Bamberg was desperately in need of the Tridentine reforms. Twice Catholicism seemed doomed in Bamberg, during the Peasants' Revolt in 1524-25 and the invasion of Albert Alcibiades in 1552-53. Between these events, however, a cautious policy aimed at avoiding conflicts with Bamberg's stronger Protestant neighbors, especially Nuremberg, provided a relative stability. This made it possible for a vigorous vicar-general to reassert control over most of the parishes within the secular jurisdiction of the prince-bishop. Ecclesiastical officials in Bamberg saw the Reformation above all as a threat to the prevailing order, and they preferred to combat it with juridical and administrative measures. Theological confrontation was left to the mendicants, whose preaching Zeissner discusses. Virtually no reform of the clergy was undertaken, partly because of the chapter's opposition.

This is a useful and convincing study based on considerable primary research, especially in Bamberg archives. Zeissner makes creative use of financial records to help illustrate the state of the Church. It would have been helpful if he had sketched in the historical background more fully as his study proceeds; the failure to do so makes it difficult at times to follow his thought and to see in perspective the ecclesiastical activity on which he concentrates. Seven documentary appendices complement the text.

ROBERT BIRELEY
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GÜNTER BUCHSTAB. *Reichsstädte, Städtekurie und Westfälischer Friedenskongress: Zusammenhänge von Sozialstruktur, Rechtsstatus und Wirtschaftskraft*. (Schriftenreihe der Vereinigung zur Erforschung der Neueren Geschichte e. V., number 7.) Münster: Verlag Aschendorff. 1976. Pp. viii, 250.

This study of the urban representatives at the negotiations for the Treaty of Westphalia goes a long way toward defining the role of the urban estate in the Holy Roman Empire. The first part of the book reviews the evolution of towns as an Imperial estate through the mid-seventeenth century, stressing the incompleteness of their transition from Crown properties (*Reichsgut*) to a true estate (*Reichsstand*). In the early modern period, when the Empire was dominated by the princes and when the territorial states grew economically at the expense of the independent towns, urban status in the Imperial Diet grew more problematic.

The central portion of this book reviews the peace negotiations at Münster and Osnabrück, where the towns encountered persistent hostility from noble estates. A serious proposal was made to introduce the Imperial Knights as a new estate in addition to, or even in the place of, the towns. The towns were forced by their weakness to concentrate on economic issues, with indifferent success. The sole urban achievements at the conferences were either negative or Pyrrhic. Towns did survive as an Imperial estate, though they could not prevent the loss of some members to territorial states. The major gain in the Peace was the confirmation of a deciding vote at the Diet in cases where the Electors and Princes were divided, but Günter Buchstab convincingly argues that this right was never universally recognized. The paralysis of the Diet after Westphalia made the vote pointless in any case. The final section of the book is an appendix reviewing the economic and fiscal condition of each of the Imperial Cities at the time of the Treaty.

Buchstab has managed to compress a great deal

of information into a few pages, and he has done an admirable job. My only criticism is that the same zeal should now be turned to the study of the territorial towns, which constituted the majority of urban communities in premodern Germany. The Imperial Cities were never the predominant form of urban commune in the Empire, outside of a few regions, and their typicality declines drastically in the early modern period.

STEVEN W. ROWAN
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JÜRGEN SCHLUMBOHM. *Freiheit: Die Anfänge der bürgerlichen Emanzipationsbewegung in Deutschland im Spiegel ihres Leitwortes (ca. 1760–ca. 1800)*. (Geschichte und Gesellschaft, Bochumer Historische Studien, volume 12.) Düsseldorf: Pädagogischer Verlag Schwann. 1975. Pp. 299. DM 38.

In the middle of the eighteenth century, Jürgen Schlumbohm argues, *Freiheit* still referred to unequally distributed privileges, the “freedoms” possessed by certain groups in the old regime; this meaning still had its defenders in 1800, but they had clearly lost ground to those who used the term to mean an equal, universal condition, the “Freedom” of the French Revolution and early liberalism. The gradual triumph of this latter usage, the author maintains, was related to profound structural changes in German society, especially to the struggles against “feudalism” and toward a “bourgeois-capitalist society” (p. 14).

This book provides us with a great deal of useful material. The author has read widely and displays an impressive mastery of a range of primary and secondary sources. His opening sections on socioeconomic developments and the emergence of a “political public” are especially well done and serve as admirable introductions to these issues. The body of the text contains a number of concise and illuminating statements on eighteenth-century political thought.

Nevertheless, it must be said that the book promises much more than it delivers. One reason for this is the author’s own conceptual apparatus. I have serious doubts about the utility of a term like “feudalism” in reference to the eighteenth century, especially when this notion is rather casually extended to include “absolutism.” (See, for example, the formulation on p. 15.) Similarly, Schlumbohm’s treatment of the *Emanzipationsbewegung* is marred by the inconsistent application of imprecise categories; I was never entirely sure what groups were to be included as *bürgerlich* and how they related to one another. There are also some problems in the terminological discussion. It seems to me that the author spends too much time

gathering examples of different ways *Freiheit* was used and not enough analyzing his sources in depth. This difficulty is magnified by the fact that much of the illustrative material has been relegated to the notes and is therefore not very well integrated into the argument as a whole. The result of this is an overall lack of cohesion and occasional superficiality.

In short, while this book tells us some interesting things about Germany in the eighteenth century, it does not satisfactorily uncover that complex and elusive web which joins semantics and social change, words and the world.

JAMES J. SHEEHAN
Northwestern University

CARSTEN KÜTHER. *Räuber und Gauner in Deutschland: Das organisierte Bandenwesen im 18. und frühen 19. Jahrhundert*. (Kritische Studien zur Geschichtswissenschaft, number 20.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 197.

Bands of robbers roamed Germany’s highways in the eighteenth and early nineteenth centuries, destroying the tranquility of local communities and challenging political authorities through acts of violence and illegality. Carsten Küther’s study focuses on the outlaws’ social origins, their lifestyle and culture, and their place in the society and state. He brings to life an important segment of the population, an underworld often overlooked by historians.

Who were the robbers? Küther discovers their origins in several social groups, all of low status or socially rejected. These included propertyless, itinerant craftsmen as well as practitioners of so-called “dishonorable” professions such as executioners, shepherds, and animal skinner. A minority of these and other social outcasts, including Jews and Gypsies, drifted into outlaw companies like the “Great Netherlands Bands” which terrorized communities throughout the Rhineland between 1790 and 1805. The robber groups developed amazingly homogeneous social patterns and an unarticulated ideology of banditry. The author describes both of these phenomena with convincing detail.

Küther acknowledges his indebtedness to Eric Hobsbawm, a pioneer in the study of rebels and bandits. But Küther refines the English scholar’s work, especially in regard to Germany. Whereas Hobsbawm argues that “social bandits” came from the peasantry, Küther finds few peasants—that is, village agriculturalists—among the German bands. They derived instead from a socially threatened rural proletariat. These German robbers nevertheless displayed characteristics of social

banditry such as distinguishing between rich and poor in their victims.

Viewing his subject as a phenomenon of "late Absolutism," Küther devotes a chapter to the outlaws' relation to political and legal authority. This is enlightening, but in emphasizing the political context of the robbers, the author overlooks important social and economic implications of his work. The prevalence of bandits—socially dislocated rebels—in the late preindustrial era suggests a society in a state of crisis. The thesis of social and economic dislocation is present in Küther's study, but its implications for the society as a whole are not systematically explored as they should be. This notwithstanding, *Räuber und Gauner in Deutschland* is a welcome contribution to our understanding of the world of the late eighteenth century, and it lives up to the usual high standards of its series.

MARION W. GRAY
Kansas State University

BERNHARD MANN. *Die Württemberger und die deutsche Nationalversammlung, 1848–49.* (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 57.) Düsseldorf: Droste Verlag. 1975. Pp. 453.

This volume deals with the relationship of the people and government of Wurtemberg with the German National Assembly of 1848–49. As the author rightly remarks, German history cannot be understood solely in terms of central institutions; in 1848 the states, particularly the larger ones, provided the reality and the concept of German unity with a hope for the future. Furthermore, it is insufficient for an understanding of the period to focus simply on the attitude of the two largest powers, Austria and Prussia, to the German unification movement. There are a number of reasons why this detailed study of the relationship between Wurtemberg and the German National Assembly as well as the Provisional Central Power is particularly welcome. In Wurtemberg there had been considerable political activity in the post-Napoleonic period, accompanied by a gradual awakening of political consciousness reaching well below the bourgeois middle class. Unable to carry its particularism to the extent of which Bavaria—with its more impressive diplomatic history and greater economic strength—was capable, Wurtemberg was an example of a South German state responsive to the call of German unity while not neglecting its own interests. Indeed, Bernhard Mann shows in his skillful treatment of a complex relationship that German unification appealed to liberals in the kingdom partly as a means of reforming the constitutional system in their own

territory, for instance through the stipulation of Basic Rights for the whole of Germany to which the individual states would have to conform. Against this background the policy of the leading "March minister," the Stuttgart lawyer Friedrich Römer, is explained here much more satisfactorily than had previously been possible. The reader is enabled to understand how this prominent liberal politician, who combined his position in Stuttgart with an active part as a member of the German National Assembly, found himself closing down the Rump Parliament. Mann gives much insight into the policy of King William I of Wurtemberg, and provides a detailed examination of the development of political associations and the press. He also pays attention to the aims of the two Christian churches.

The detailed archival research leads him to certain broad conclusions. The haste with which the *Bundestag* was discarded is seen as a hindrance to German unification. Mann admits that members of the Frankfurt Parliament lived in a world of their own, somewhat divorced from the realities of the general political and diplomatic situation, but defends them against charges of inefficiency. The magnitude of the task facing German liberalism in the period 1848–49 is fully recognized. While he makes no attempt to minimize the differences between moderate liberals and radicals, he emphasizes their many common aims.

FRANK EYCK
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FELIX GILBERT, editor. *Bankiers, Künstler und Gelehrte: Unveröffentlichte Briefe der Familie Mendelssohn aus dem 19. Jahrhundert.* (Schriftenreihe Wissenschaftlicher Abhandlungen des Leo Baeck Instituts, number 31.) Tübingen: J. C. B. Mohr (Paul Siebeck). 1975. Pp. lii, 328. DM 87.

An extraordinary breadth of historical perspective can be gained from this collection of unpublished letters of the Mendelssohn family. The letters derive from the estate of Benjamin Mendelssohn (1794–1874) and from copies made in other collections during the 1930s. Felix Gilbert, who shares such lineage himself, has exercised a perceptive editorial hand in selecting letters from many lines of the family, letters which show the fabric of its life in close relationship to cultural and political tendencies of the nineteenth century. He chose on the basis of content rather than the authors' renown. The letters from between 1806 and 1848 include only a few of Felix Mendelssohn and Dorothea Schlegel, and those from between 1848 and 1888 (about half the book) break new ground in a period of the family's history when it did not have such illustrious members. A final section consists

of correspondence from scholars outside the family to Benjamin, a geographer, and his nephew Karl Mendelssohn-Bartholdy, a historian. The volume provides helpful tools in a biographical appendix and a pair of detached genealogical charts.

Helpful, too, is Gilbert's broadly conceived introduction. It shows that the Mendelssohns—"bankers, artists, and scholars" all—remained apart from the Prussian bureaucratic elite until the end of the century. In their politics they adhered to moderate liberalism linked with loyalty to the monarchy. In their religion they showed varied postures of tolerant Judaism, state-oriented Protestantism, and militant Romantic Catholicism. Gilbert explores their religious and personal conflicts frankly.

The letters should interest historians in many fields. Rich description of social and cultural life appears in those written in the early decades of the century from Paris, London, Rome, and the family's base in Berlin. Vivid glimpses from behind the scenes of intellectual history turn up in anecdotes about Ranke, Humboldt, Schelling, and Hegel (Marianne Mendelssohn reports playing billiards with the latter). Extensive commentary on German politics—church affairs as well as national unification—dominates the letters from after mid-century, most significantly those of Gierke, Treitschke, and Moritz von Bethmann-Hollweg. And letters from Humboldt demonstrate his close ties to the family and assistance to the cause of Jewish legal rights.

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ALAN PALMER. *Bismarck*. New York: Charles Scribner's Sons. 1976. Pp. x, 326. \$12.50.

Alan Palmer rightly contends that a new general biography of Bismarck is justified by the large number of books and articles on aspects of his life which have appeared in recent years. (The most widely used general account, that by A. J. P. Taylor, is now more than twenty years old.) Those seeking a well-written, balanced, and reasonably complete introduction to the subject will find it here in a most attractive format, including extensive footnoting, a useful bibliography, eighteen well-chosen illustrations, two maps, and a superb index.

Palmer is a modest writer, deliberately refraining from constructing an "interpretation" of Bismarck's life. He prefers to tell the story as straightforwardly as he can, utilizing recent scholarship on specific points, letting the facts speak for themselves. The dryness of this approach is relieved by the author's gentle wit and by numerous apt quo-

tations from contemporary sources (since published and mostly long available in German). Palmer's use of the neglected diaries of Crown Prince Frederick William and of the correspondence of his wife sheds needed light on the significant, if often frustrating, role of the Crown Prince during most of Bismarck's time in office.

Bismarck's letters to his wife Johanna reveal a more sensitive and more attractive side of his character than is suggested by his better-known political utterances. The failure of Palmer to adequately explain the connection between the private and the public Bismarck may indicate where future work needs to be focused. One hopes that further research and further evaluation of the material now available will allow a more flavorful interpretation of the great man to emerge. The earlier versions of Emil Ludwig and of Taylor needed revision, but they may have captured the animus of Bismarck more nearly than has the cautious Palmer.

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MARIE-LOUISE PLESSSEN. *Die Wirksamkeit des Vereins für Socialpolitik von 1872-1890: Studien zum Katheder- und Staatssozialismus*. (Beiträge zur Geschichte der Sozialwissenschaften, number 3.) Berlin: Duncker und Humblot. 1975. Pp. 134. DM 38.60.

The *Verein für Socialpolitik*, founded in 1872, was an organization of German social scientists, mainly academic teachers, with some participation by government officials and enlightened businessmen. Its members, though their politics were different, were united by their commitment to social reform in opposition to laissez faire. The organization tried to exert influence mainly by publishing factual reports and analyses of social conditions, not unlike the Fabian Society in Great Britain, although without the latter's tendency to identify with the labor movement and to take political action. In the later phases of its existence, the commitment of the *Verein* became weaker and it finally resembled simply a professional organization of economists.

Marie-Louise Plessen has chosen as her subject the first two decades of the *Verein's* history, when its activity was of crucial importance for the origin of Bismarck's social welfare legislation. The book contains quite a few interesting details about the crosscurrents in German thought about social questions in the late nineteenth century, but the analyses have little value. Plessen criticizes the *Verein* for being *systemerhaltend* (p. 11), i.e. trying to preserve the social system by correcting misuses rather than working for a completely new social order. But she fails to realize that her distinction

between system-preserving and system-changing reforms is largely irrelevant, because most reforms tend to improve the chances for the existing system to survive and at the same time operate for the change of the system, one reform usually laying the foundation for the next. Basically, at least by implication, Plessen faults the *Verein* for not adopting revolutionary Marxism; she seems to assume that the organization, if it had done so, would have been more effective, although the impotence of German Social Democracy in its revolutionary-Marxist period proves the opposite. Moreover, Plessen has bitten off more than she could chew: she wanted to give a picture of the origin of the ideas of social reform in Germany prior to the founding of the *Verein*, and also to put its activities within the framework of Bismarckian Germany. Laudable as these intentions are, even a more discerning and less biased author could not have accomplished that in a satisfactory manner within a volume of 134 pages.

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DAN S. WHITE. *The Splintered Party: National Liberalism in Hessen and the Reich, 1867-1918*. Cambridge, Mass.: Harvard University Press, 1976. Pp. viii, 303. \$14.00.

Lack of reformist zeal invariably springs to mind as the central problem of the National Liberal Party in the German Empire. Until now historians have explained it by focusing on theoretical statements, campaign rhetoric, values, and parliamentary tactics of leading personalities. The existence of autonomous regional parties in the Empire, however, has inspired Dan S. White to examine the experiences of the Hessian National Liberals and to place them in their local, Imperial, and European contexts. In this way he was able to combine a desire for a precise rendition of social and economic issues with non-Marxist concepts of class, power, and interest groups to realize a new approach to National Liberal studies.

White depicts the domination of the National Liberal Party by its diverse regional groups as the chief cause of its failure to develop any reformist zeal. In the Hessian case the National Liberals turned into a popular but conservative interest group in the early 1800s. They supported the preservation of Hessen's undemocratic suffrage system—which won backing from the notable class—and specific legislation such as fiscal and protective tariffs which met concrete social and economic needs of a broad spectrum of the local electorate. This development, however, immediately became nationally significant because the party had fallen into disarray after Bismarck broke

with it over the protective tariff. Naturally when the party revived in 1884, the Hessians played a role in formulating the new Heidelberg program, and the further development of any major reform policy seemed conditioned upon its acceptance by local parties. Accordingly the Hessians were treated with impunity when they opposed egalitarian aspects of the Heidelberg program and estate tax and financial reforms of 1909.

White adds a dimension of complexity to the study of the National Liberal Party by highlighting the difficulties it encountered in trying to retain the support of its traditional allies in the middle strata of society. He distracts his reader from this achievement, however, by trying to accomplish more. His analysis exaggerates the importance of the regional party on the issue of reform by ignoring forces for party unity. The latter, however, provide a perfectly sufficient explanation for any lack of reformist zeal. On questions of reform National Liberal intentions were invariably limited by prior commitments to nationalism, the expansion of state authority, military budgets, imperialism, support for Bismarck, suppression of the Social Democratic Party, and the interests of heavy industry and big agriculture.

Further problems stem from his incomplete treatment of questions of party organizational structure. After 1900 the new national organization consisting of a secretariat, executive committee, and party congresses influenced party policies, programs, and leadership, but White fails to assess the role of the regional parties in this organization. He also does not identify the position of various organizations and financing in the structure of the Hessian party after 1890. These omissions might have been easily corrected had he consulted various papers in the archives of the German Democratic Republic and the works of the Jena historian Herbert Schwab whose dissertation on the National Liberals contained much relevant information. Finally, White sometimes puts a strain on his readers stylistically. He too often uses social-science jargon, the same word to express different meanings, abstract nouns, and nouns as adjectives.

Despite these criticisms, his work should well serve to renew scholarly interest in a generally neglected but vital subject.

JOHN F. FLYNN
University of the South

PETER SCHUMANN. *Die deutschen Historikertage von 1893 bis 1937: Die Geschichte einer fachhistorischen Institution im Spiegel der Presse*. Göttingen: Selbstverlag des Verfassers. 1975. Pp. vi, 459.

By the end of the nineteenth century, the German historical profession was a "staatserhaltend" pillar of

the official imperial order. In his study of one of the profession's leading forums, the nineteen meetings of the Convention of German Historians held between 1893 and 1937, Peter Schumann precisely documents the conservatism, both methodological and political, of German historiography. Through detailed analysis of the scholarly papers read at these meetings and the press coverage they received, Schumann reveals that German historians studiously avoided methodological and theoretical controversy, insulating themselves from any challenge to their traditional methodology which treated history primarily in terms of the nation-state. But *Historikertage* sessions were not merely political in approach—they also became increasingly politicized. The research topics chosen by scholars and the conclusions they drew mirror an unmistakable monarchical conservatism and growing nationalism. Still subtly veiled by professions of detached objectivity at the turn of the century, this conservative jaundice within the discipline became more manifest as the war approached and emerged blatantly after 1918. By the 1930s even the pretense of scholarly detachment had been abandoned; Weimar *Historikertage* were merely vehicles used to present historical "proofs" of Germany's innocence of war guilt, the necessity of *Grossdeutsch* solidarity and *Anschluss*, or the need for vigilance against Slavic aggression. This organ of the historical profession was so politicized, antirepublican, and pan-German that a Nazi *Gleichschaltung* of the *Historikertage* after 1933 was unnecessary.

Schumann's study of the products of German historical scholarship corroborates the conclusions which Iggers drew from his examination of the historians' philosophical conception of history: German historians, despite their aura of scholarly detachment, seriously lacked critical distance from the prevailing values and political passions of their time. Schumann's findings suggest incidentally that the impact of the Lamprecht debate upon the profession has perhaps been overrated, and that *Grossdeutsch* revisionism among Reich historians was not a direct result of post-1918 desires for *Anschluss*, but had begun already before the war. Unfortunately Schumann's scholarship fails to consider recent American studies on the topic (McClelland, Ringer, Suval). The insulation of German historians from other disciplines and academic currents which Schumann exposes for the period 1893 to 1937 has apparently not yet been completely overcome.

GARY D. STARK
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THOMAS NIPPERDEY. *Gesellschaft, Kultur, Theorie: Gesammelte Aufsätze zur neueren Geschichte*. (Kritische

Studien zur Geschichtswissenschaft, number 18.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 466. DM 64.

Though published previously in slightly different versions, the sixteen studies in this collection present the outlines of a new position in German historiography. Thomas Nipperdey is best known for the painstaking *Die Organisation der deutschen Parteien vor 1918* (1961) and his more recent attempt to synthesize the intellectual and social history of the sixteenth century in *Reformation, Revolution, Utopie* (1975). The present work retraces some of the same ground but also sets out in quite different directions. It proceeds from the theoretical plane ("On Relevance," "The Anthropological Dimension in History," and "Historicism and Its Critics Today") through "social-functional" analyses of ideas, attitudes, and institutions (utopias, anti-Semitism, voluntary associations, national monuments, and primary schools) down to specific figures, events, and movements (the political theology of C. J. Hundeshagen, the "verdict" on 1848, youth and politics in 1900, and the *Deutsche Studentenschaft* in the early Weimar years). Despite the several levels and diverse subjects, the collection holds together in a way worthy of the series in which it appears.

Nipperdey wants to move German historiography beyond the sterile extremes of moralizing critique versus apologetics—or in the formal sense, structural analysis versus narrative. His remedy lies with what he calls an "anthropological perspective" to be constituted between traditional sociology, psychology, and history. The object of this approach is the "triangular relation of society/culture/person." If this sounds unobjectionably up-to-date, Nipperdey goes on to challenge the current "neo-critical" commitment to the present and "praxis"; the transformation of history into "political pedagogy" he finds misconceived and ultimately counterproductive. Insisting that "the sociology of a science is not its logic" (p. 260), Nipperdey holds out for what he calls "open objectivity": the historian cannot claim to be prosecutor, judge, and law-giver all in one. Accordingly he attempts a selective rehabilitation of certain older historicist tenets in the service of a new kind of social history of ideas and attitudes.

The author has little taste for either sweeping schemas or dallying brushstrokes. He protests against the thesis of uniform continuity to 1933 and stresses that there are always multiple continuities in history. A review of Hans-Ulrich Wehler's *Das deutsche Kaiserreich* (1973) is devastating and in places intemperate: Wehler is branded as a "new Treitschke" under different colors, cramming and eliding as suits his overall purpose. Nipperdey de-

mands sharper conceptualization and more careful differentiation in German historiography; his own studies show a fondness for rather elaborate typologies and distinctions. The net effect is to spread the balm of "critical rehabilitation" on some very sore points—the 1848 liberals, the *Gemeinschafts*-ideal, the youth movement, and others. The author insists that there were "emancipatory" and "modernizing" currents in German history and he suggests that certain precepts of historicism may still have a function both in discovering and advancing them.

H. MICHAEL ERMARTH
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LEO HAUPTS. *Deutsche Friedenspolitik, 1918–1919: Eine Alternative zur Machtpolitik des Ersten Weltkrieges*. Düsseldorf: Droste Verlag. 1976. Pp. 489.

This lucid, well-researched book makes an important contribution to the literature on "continuity" by emphasizing the magnitude of the break between Germany's 1918–19 peace proposals and its prewar foreign policy. The men around shipping magnate Alfred Ballin and banker Max Warburg had argued for years that Germany would only prosper in a world of international free trade. The war confirmed them in their views by revealing Germany's critical dependence upon foreign trade. *Mitteleuropa* was inadequate for the economic needs of industrial Germany, and even military victory would not assure German access to world markets. It was imperative, therefore, that the economic war end with the shooting war. Since economic growth based on international trade would only further German industrialization, no future German government could dispense with the cooperation of labor and the Social Democrats. The future Germany would therefore be a liberal, democratic state relying for its access to world markets on international free trade, freedom of the seas, and international arbitration rather than upon the military power politics of the prewar era. Such views could not break through into German policy until the German military collapse, and Leo Haupts details the story of that breakthrough.

The Social Democrats recognized that if the allies could be persuaded to allow food imports and German economic reconstruction, the legitimacy of social democratic leadership would be assured. They therefore relied upon the financial expertise of Warburg's group in dealing with the allies. The persistence of the allied blockade and the allied peace terms came as a great shock: they undermined efforts at a liberal reconstruction, gave new life to heavy industry and the Pan-Germans, and forced even Warburg's group into a revisionist stance. Nonetheless, Warburg and his financial experts at the peace negotiations were convinced

that economic realities would bring the allies to reason. They expected, moreover, that the allies could not enforce the peace without the cooperation of the German government. Unlike Ebert and Erzberger, who saw no alternative to signing, they were therefore prepared to take the extreme step of rejecting the allied draft.

The harshness of the allied terms, argues Haupts, by pushing all Germans into a revisionist and defensive posture, obscured Germany's genuine desire for a peace of conciliation and the magnitude of the break between Germany's new foreign policy and prewar *Weltpolitik*. "In the emotion laden atmosphere of June 1919," he concludes, "it was clear to only a few that resistance too could be a means of conciliation." Throughout, Haupts perceptively illuminates the interplay between foreign policy, domestic politics, and economics. A useful nineteen-page statistical appendix summarizes Germany's prewar, wartime, and postwar economic position.

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DAVID W. MORGAN. *The Socialist Left and the German Revolution: A History of the German Independent Social Democratic Party, 1917–1922*. Ithaca: Cornell University Press. 1975. Pp. 499. \$19.50.

ROBERT F. WHEELER. *USPD und Internationale: Sozialistischer Internationalismus in der Zeit der Revolution*. Translated by AGNES BLÄNSDORF. Frankfurt am Main: Verlag Ullstein. 1975. Pp. iv, 384.

Aborted revolutions seldom attract as much attention as successful ones. An exception is the turmoil in Germany from 1918 to 1923. Less severe unrest during the 1960s in highly industrialized societies led many scholars to reopen the question—long answered negatively—whether revolution has been possible in Western Europe in the twentieth century. The works under review make differing assumptions about how close Germany came to a true revolution in the period shortly after World War I.

Since much of what follows is critical of both books, it should be emphasized that they are solidly crafted monographs based on wide reading in the sources, including much archival material. They and Hartfrid Krause's *USPD: Zur Geschichte der Unabhängigen Sozialdemokratischen Partei* (1975) are the first extended studies of the Independent Socialist Party (USPD) published during the past half-century.

The USPD was produced by the fragmentation of the German working-class movement during World War I. Similar splits occurred in other countries; the one in Germany is striking because the enormous Social Democratic Party (SPD) had been the only working-class political organization

of consequence. Three major groupings emerged, although lines between them were fluid. The SPD, whose leaders were known to their opponents on the left as "social patriots," accepted the bourgeois nation state. the USPD stood for internationalism, was suspicious of the nation state, sought to end the war without delay, and hoped for a revolution of some sort. Another grouping, which included the Spartacists and subsequently the Communist Party, stressed the need for revolution. Although some of the USPD's leaders took a hand in governing Germany after the Armistice, the party continued to be very critical of the SPD. In the fall of 1920 a majority of the delegates to a special USPD congress voted to enter the Comintern—a decision favored by the rank and file, but opposed by much of the party hierarchy. A substantial segment of the USPD soon merged with the German Communist Party. Two years later most of the remnants of the USPD fused with the SPD.

Robert F. Wheeler depicts the USPD sympathetically as a "third force." He believes that the USPD and the Workers' and Soldiers' Councils (*Räte*) could have forged a desirable alternative to the Weimar Republic. His views are similar to those of the scholars whom Brian Peterson has aptly dubbed "the *Räte* school" because they underscore the thwarted potential of the council movement to create a polity superior to parliamentary democracy while avoiding Bolshevik-style revolution (*New German Critique*, [Winter 1975]:114).

The challenging theme of Wheeler's book is socialist internationalism, but his diligent attempt to relate the failure of the USPD to this subject is unconvincing. After discussing such topics as the party's limited geographical basis, its lack of appeal to women, and its tendency to attract younger workers, he puts the burden of blame on Moscow. He terms "artificial" the issue that split the USPD in 1920—entry into the Comintern (p. 214). This interpretation wishes away the Russian Revolution and overestimates the revolutionary potential of the USPD. It is as if one were to blame the Chinese Communist debacle of 1927 on the Comintern or the Russian Revolution; of course the Comintern played an important role in this failure, but without the Russian Revolution and the Comintern the world balance of forces would have been even less auspicious for revolution in China. During the course of a real revolution in Germany the USPD, like most parties in most revolutions, would have been transformed or destroyed. Was not the ebbing strength of the socialist Left due more to containment of the Russian Revolution in 1919–20 and to contradictions in the USPD than to what Wheeler regards as misplaced hope in the Comintern's internationalism?

David W. Morgan's work rests on a less problematical conception of the German revolution of 1918–19: he finds no possibility of a "third way," and for him the Weimar Republic had to be a bourgeois order with a parliamentary government. Because the USPD was unhappy with such a regime, it could play no positive role after the Armistice halted the war and thereby destroyed the party's *raison d'être*.

This reading of history, frankly partisan toward the bourgeoisie, makes much sense, but Morgan supplies faulty underpinnings for it. He tells us that the determinants of mass political behavior are "attitudes, formulas, and traditional modes of action" (p. 13). Although this notion has much to recommend itself when applied to an unsuccessful revolution and to a party (the SPD or USPD) not transformed or destroyed by a revolution, it is much less useful in analyzing the development of the Bolsheviks or Fidel Castro's July 26 Movement.

Another weakness in Morgan's work is his impatience with theory, a distinct handicap for the historian of a party in which ideology was taken very seriously. Morgan complains often of the USPD's adherence to "the ideology of the class struggle" and denounces this "ideology" as "a factor in the corrupting circumstances surrounding the birth of the Republic and preventing its consolidation" (p. 446). For Morgan, Weimar was scarred from the beginning, not because the USPD and the council movement failed, but because the Left refused to embrace warmly a bourgeois order. He finds the influence of the USPD in postwar Germany insidious due to the susceptibility of many workers to its sophistries: "A vague, conditional belief in parliamentary democracy was perhaps more characteristic of the bulk of the SPD rank and file than the complete commitment that dictated [the SPD leader Friedrich] Ebert's course. . . ." (p. 130).

This statement gives a telling glimpse of the direction of Morgan's interpretation. His repeated complaints about the dogmatism, authoritarianism, and class-consciousness of socialists suggest a view of twentieth-century Germany in which the real culprits are the Left with some backhanded assistance from the Right.

Morgan's book has several useful appendices, including eleven pages in fine print of biographical sketches of leading members of the USPD. The index is superb. Students of the Weimar Republic and German party history are indebted to both Morgan and Wheeler. The latter's book is to date available only in German translation.

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MARJATTA HIETALA. *Der Neue Nationalismus: In der Publizistik Ernst Jüngers und des Kreises um ihn, 1920–1933.* (Suomalaisen Tiedeakatemia Toimituksia, Annales Academiæ Scientiarum Fennicæ, Sarja, Ser. Bnde, volume 194.) Helsinki: Suomalainen Tiedeakatemia. 1975. Pp. 274. 55 MK.

ECKHARD WANDEL. *Hans Schäffer: Steuermann in wirtschaftlichen und politischen Krisen.* (Veröffentlichung des Leo Baeck Instituts.) Stuttgart: Deutsche Verlags-Anstalt; distributed by Leo Baeck Institute, New York. 1974. Pp. 378.

DIETER REBENTISCH. *Ludwig Landmann: Frankfurter Oberbürgermeister der Weimarer Republik.* (Frankfurter Historische Abhandlungen, volume 10.) Wiesbaden: Steiner Verlag. 1975. Pp. viii, 321. DM 48.

Of these books Eckhard Wandel's biography of Hans Schäffer should have been the most significant one. Schäffer was a top government official throughout most of the Weimar era, first as *Ministerialdirektor* in the Reich economics ministry, later as *Staatssekretär* in the finance ministry. Deeply involved in reparations and budgetary problems, he was the confidant of chancellors, ministers, Reichstag deputies, bankers, and industrialists. He was also, happily for the historian, a tireless diarist, correspondent, and writer of memoranda.

Unfortunately the book does not live up to the expectations which such credentials inspire. The largest part is simply a superficial resumé of Weimar political and financial history. There are tantalizing references to behind-the-scenes talks with political and economic leaders both in Germany and abroad, but we are rarely told what was discussed on these occasions. Yet the two entries from Schäffer's diaries reprinted in an appendix—one an eerie conversation with Chancellor Brüning, the other a rather pathetic talk with Reich President von Hindenburg—suggest that the diaries (twenty-four volumes for the Weimar period alone) are a goldmine of information on problems and personalities of that era.

The specific impression one gains is of a lack of concern on the part of a man with Schäffer's liberal, humanistic credentials with the hardships that the deflationary policies of the Brüning government imposed on the German people. Even years later he still felt that that policy, geared to convincing the Western powers of Germany's inability to pay its reparations debts, was correct despite its disastrous human and political consequences. Perhaps little more could be expected from someone who argued that wage reductions did not decrease purchasing power, but merely shifted it to other pocketbooks. Yet on a personal plane Schäffer was not a cold-hearted man. When

he was called upon later as a private citizen to come to the aid of Nazi victims, he gave generously of his time and money.

Unlike the Schäffer study Dieter Rebentisch's biography of Ludwig Landmann, *Oberbürgermeister* of Frankfurt am Main during the greater part of the Weimar years, is an informative, fact-laden account of the aspirations and problems of a major German city during that time. Rebentisch tells of Frankfurt's pioneering role in building one of the first municipal airports in Germany and of Landmann's ambitious plans for the construction of an *Autobahn*—a project not realized then but carried out some years later by Hitler.

The book also provides useful data on the problem of American loans to German municipalities. New light is thrown on the role of Reichsbank President Schacht. His attempts to block such loans as inflationary in the short range and impossible to repay in the long run forced cities like Frankfurt to take up those short-term credits that wrought havoc with their finances during the Depression. In vain did Landmann challenge Schacht's definition of productive investments, insisting that schools, hospitals, and recreational facilities were productive inasmuch as education, health, and recreation were preconditions of a productive work force.

A recurring theme of the book concerns the issue of municipal versus private enterprise. Landmann was an active protagonist of city-owned public utilities, partly to avoid monopolist pressures as in the case of gas, partly because he considered city-run enterprises as more economical from the city's perspective. But undoubtedly Landmann also looked upon Frankfurt as something of a personal empire which he wished to make as independent and strong as he could.

During the early nineteen-thirties Frankfurt presented a miniature replica of the Weimar Republic, with the same problems of parliamentary stalemate and recurring financial crises that plagued the national government. Authorized by the Hindenburg/Brüning emergency decrees, Landmann assumed increasing administrative powers; but without a corresponding initiative on the national plane, his attempts to revive building activities and create public-service jobs remained ineffective.

Both Schäffer and Landmann were Jews, but their post-Weimar careers ran very different courses. Schäffer left Germany for Sweden shortly after Hitler's appointment as chancellor and thanks to his international contacts had another distinguished career as legal adviser to the Kreuger match trust. Landmann, worn out and plagued by ill health, finally found a refuge in the Netherlands; but when the Germans took over

that country, he had to live out his life in a hideout provided by courageous Dutch friends.

Ernst Jünger, the subject of Marjatta Hietala's book, had nothing in common with Schäffer and Landmann except contemporaneity. Hietala examines the writings of this "new nationalist" and those of his close associates in both qualitative and quantitative analyses. The qualitative analysis adds no new insights to the voluminous literature about Jünger; that of selected publications of his fellow "new nationalists" reveals a few departures from the views of the master, which may help to explain why some of these men joined the Nazis whereas Jünger as a "political soldier" did not.

The quantitative analysis merely confirms the qualitative conclusions. As a skeptical non-expert I cannot help wondering whether in investigations like this one the quantitative approach can really be helpful; it does seem to drain the human element out of such studies—an impression reinforced in this case by the book's pedestrian style.

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DIETER GESSNER. *Agrarverbände in der Weimarer Republik: Wirtschaftliche und soziale Voraussetzungen agrar-konservativer Politik vor 1933*. Düsseldorf: Droste Verlag. 1976. Pp. 304.

Until the middle of the last decade, the politics of Germany's agricultural community during the Weimar Republic remained virtually unexplored. Since then, major steps have been taken to remedy this neglect, for the most part by a new generation of German historians. Dieter Gessner, born in 1940 and currently a Lecturer at the Technische Hochschule in Darmstadt, belongs to this new group.

The major theme in *Agrarverbände* consists of conservative agrarian political responses to the continually deepening crisis that enveloped the agricultural sector after 1925. This theme is joined to two significant theses. The first argues that the transformation from empire to republic left the agricultural sector virtually unaffected. Despite superficial alterations, continuity was visible in patterns of land ownership, the leadership of traditional elites, the existence of established political parties and interest organizations, and in unceasing demands from producers for tariff protection, easy credit, and advantageous tax arrangements. This continuity was important, Gessner argues, because the problems from which prewar agriculture had suffered, the organizations and parties through which it operated, and the solutions that its spokesmen proffered, were carried over into the Republic. Gessner's second thesis involves the content of the conservative agrarian responses under investigation. Whereas traditional views treat con-

servative agrarians as devoted to the destruction of the Weimar Republic, the author demonstrates that two conservative agrarian factions coexisted in bitter conflict with one another until the demise of the Republic. The first supported the Republic after 1925 in hopes of procuring ever-increasing government assistance for agricultural producers; the second conservative agrarian faction pursued an ideologically motivated policy of non-cooperation after 1928, if not earlier, in hopes of forcing changes in the existing form of government in an authoritarian direction. Only when the moderates were unable to resolve the agricultural crisis with Brüning's assistance, Gessner argues, did the intransigents, in alliance with the Hitler party, finally gain the upper hand.

This book does have its misleading points. While covering the interorganizational struggles which occurred within the leadership of the agricultural sector's two major interest organizations, it virtually ignores the significant conflicts which occurred between them. In so doing, Gessner presents too strong a picture of sectoral unity. Gessner also badly underestimates the membership of the *Reichs-Landbund*, Germany's largest agricultural interest organization, thereby minimizing its predominant position within the agricultural sector. These criticisms aside, *Agrarverbände* is an important contribution to our increasing understanding of agrarian politics during the last half of the Republic's lifespan. The thoroughness of the author's research is especially noteworthy: Gessner has utilized the holdings of a dozen archives and his sources include the unpublished papers of some of the Weimar Republic's most important agrarian figures.

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PETER H. MERKL. *Political Violence Under the Swastika: 581 Early Nazis*. Princeton: Princeton University Press. 1975. Pp. xiv, 735. Cloth \$30.00, paper \$10.75.

In 1934, Theodore Abel, Columbia University sociologist, sponsored an essay contest for "the best personal life history of an adherent of the Hitler movement." As a result he obtained 581 autobiographical accounts of early adherents to Nazism. These *vitae* formed the basis of Abel's 1938 work *Why Hitler Came to Power*. Implicit in his approach was the overarching idea that certain substantive conclusions regarding motivation could be drawn on the basis of a quantitative technique. Not until recently, however, have technological advances made possible a truly sophisticated analysis of Abel's material. Peter Merkl's *Political Violence Un-*

der the Swastika: 581 Early Nazis is such an analysis, and the historical profession is indebted to him for it.

Through a most skillful use of cliometrics, the author is able to call into question several previously accepted assumptions concerning National Socialism: that it was mainly a revolt of the lower-middle class; that it was a rebellion against modernity; that there was a particular variety of "authoritarian personality" *à la* Adorno that can be identified as the archetypal follower; and finally, that ideological motivation was of primary importance in determining action. According to Merkl's thoroughgoing and sensitive analysis the "average" Nazi could be the product of an authoritarian or a permissive home; he or she could belong to an upwardly mobile, to a stagnant, or to a declining social class. In fact, for Merkl, the question of spatial mobility, or lack of it, is a far more important issue, at least insofar as political violence was concerned. The question of ideology's providing a readily discernible motive for action is shown to be a vexed one.

Indeed, the upshot of Merkl's work would seem to be that there really was no archetypal Nazi, but rather several varieties, each with a somewhat singular admixture of class, familial, and ideological backgrounds and influences. The author classifies his subjects by type of political activity (Marcher-Fighter, MF; Marcher-Proselytizer, MP; and Marcher-Fighter-Proselytizer, MFP) and separates the early Nazis into several "generations" (prewar, war, "victory-watchers," Weimar "youth revolt," etc.). Each variety of political activist and/or "generation" or "cohort" had its own level of politicization and its distinctive psychic makeup.

Merkl's approach, one in which the post-World War I "fall of . . . German National pride" and the Weimar "youth revolt" appear to be the most important of the historical circumstances responsible for the rise of the Hitler movement, offers an answer to the question posed by Wolfgang Sauer: did the Nazi movement have a true revolutionary potential? His answer will hardly please investigators who, taking a Marxist or quasi-Marxist approach, attempt to tie Nazism to particular classes and socioeconomic motives. Indeed, while most National Socialists did come from what could be called (very roughly) a "middle-class" background, Merkl seems to have demonstrated that, in its claims to be a super-class movement, the Nazi party was not being consciously disingenuous.

What emerges from the painstakingly executed charts and graphs and from Merkl's extremely lucid prose is a picture of a *Sammelpartei* that succeeded in doing what Germany's forlorn liberals

had been calling for since 1848—establishing a powerful nucleus for the *Volksgemeinschaft*. The ideational hollowness of Merkl's Nazi Party calls into question any sort of cultural-criticism approach to it, to say nothing of one concerned directly with ideology, while the socially ecumenical nature of the phenomenon discourages any sort of investigation conditioned by socioeconomic determinism in general and Marxism in particular. Indeed, Merkl's persuasive analysis pushes us to a point at which the efficaciousness of all traditional methods of historical *explanation* must be called into question. We are apparently, then, confronted with a disquieting choice: either we can fall back upon the prosaic Rankean dictum that the historian's task is to describe the historical event "*wie es eigentlich gewesen ist*," or we must concede that the analysis of National Socialism falls within the purview of disciplines other than history, as traditionally defined.

There is a solution and, perhaps unwittingly, it is offered by Merkl himself. Though he downplays the role of ideology in the National Socialist movement, he maintains the importance of anti-Semitism. He does not, however, consider anti-Semitism to be of ideological significance. "Antisemitism [sic] is no more an idea or an ideology than a common obscenity is. It is a state of mind that comes in different shadings and degrees of intensity which are likely to be highly related to other attitudes and to behavior" (p. 448). If it is held that there was nothing ideological about this key idea, then it must follow that ideology has but little to do with motivation for Nazi actions. But for those who, as the author puts it, "fought day and night" for the Nazi Party or who rose to prominence in its ranks (see pp. 503, 611, and 628 in particular) *anti-Semitism* was indeed of primary importance. "Office-holders of *all levels* tended heavily to be attracted by the ideology and by ideological propaganda. . . . It seems clear that the more ambitious office-holders, unlike the stormtroopers, had a strong ideological motivation" (p. 628, my emphasis). Thus we can conclude that, while ideology may not have been of great importance for the brutalized street-sluggers of the SA, and perhaps not even for the *majority* of National Socialists, it most certainly was important for those who would hold offices within the party. Yet, Merkl would suggest that this "ideology" really was no ideology at all.

Implicit in the author's dismissal of ideology as being of central importance in National Socialist actions is the notion that an ideology must offer a consistent program for political and social action. Yet, *au fond*, is ideology *ever* anything more than a sort of mnemonic shorthand of commonly held social attitudes and beliefs? Can one say with cer-

tainty that any ideology, e.g., liberalism, Marxism, Gaullism, etc., ever has succeeded in overcoming internal inconsistencies, much less in translating its complete ideational contents into social and political reality once it has attained power? One can, of course, still persist in the suggestion that an ideology's substructure must consist of a belief or nexus of beliefs, but to insist that the reification of prejudice visible at the heart of National Socialist thought cannot be viewed as an "ideological" phenomenon is to call into question *any* effort to determine how espoused ideas, however obscene these ideas might be, reflect social attitudes. We are not justified in concluding that what is obscene is not or cannot be ideological. Indeed, the paramount importance of ideology, or, at the very least that idea which was central to Nazism, is demonstrated by Merkl's showing it to be the most important *conscious* motivational factor in determining the activities of office-holders in the Nazi party.

Another problem involving ideology is most clearly illuminated in Merkl's study. He describes office-holders as, besides being the most ideological in their orientation, also being the most "paranoid." Merkl is not using this phrase sloppily or loosely. Indeed, despite his calling into question the Adorno "authoritarian personality," he is very much concerned with the necessity of arriving at some sort of understanding of the Nazis' psychic makeup(s). With such an approach it is obvious that the role of ideology once more can be reduced to secondary importance. Even granting that, ideology may be studied as representing a *conscious* rationalization for actions. *Why* are certain varieties of rationalizations chosen? What are their forms and with what contents are they filled? It is here that the *psychohistorian* must be called upon to perform a crucial function. Ideologies, even if one suggests that they are reifications or rationalizations of transhistorical pathological conditions, are nevertheless *zeitgebunden*. National Socialism was a twentieth-century phenomenon in both form and content, though the problems it represents might well have been supplied by phylogenesis. The psychologically informed historian must address himself or herself to the awesome task of determining why a certain ideology was chosen to rationalize particular drives and of correlating the contents of the ideology to social, cultural, and political conditions. The synoptic judgment inherent in this method can be viewed as the very essence of historical understanding.

Merkl's work can leave one with the disquieting notion that the National Socialist movement cannot be described "historically," or at least not with the traditional impedimenta of historical research. Words such as "totalitarian," "authoritar-

ian," "idealism," "nihilism," and, of course, "ideology," suddenly no longer seem to mean anything, or at least there appear to be new meanings, the applicability of which within the historical discipline must be uncertain. In having these effects, *Political Violence Under the Swastika* challenges us to investigate with perhaps unwonted rigor some of our primary assumptions about National Socialism. More than that, though, it pushes us to make some hard choices regarding future investigations of this movement. For it is obvious that if future historians of Nazi Germany are to be seriously concerned with what the Nazi phenomenon in fact meant, they will not be able to view such terms as ideology through liberal glasses. Perhaps, if one wants to be thoroughly reductionist about it, one can say that basically the Nazis were not ideologists (just as nobody basically is). Nevertheless they thought that they were or, perhaps more important, *wanted* to think that they were. Why this was the case and *how* and why a particular ideology best served the needs of particular people at a particular place and time are the questions the historian—more particularly, perhaps, the *psychohistorian*—must be willing to confront.

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Bundesrepublik Deutschland und Berlin. Volume I, Berlin: Eine geographische Strukturanalyse der zwölf westlichen Bezirke. by BURKHARD HOFMEISTER. (Wissenschaftliche Länderkunden, volume 8.) Darmstadt: Wissenschaftliche Buchgesellschaft, 1975. Pp. xx, 468.

The fascinating contents of this book are of far greater relevance and interest to historians than the rather formidable title might suggest. The goal of the series on the Federal Republic and West Berlin is no less than to provide the first systematic and thorough treatment of the geography of Germany since World War II. It is intended specifically not only to bring traditional geographical information up to date, but to incorporate new methods of analyzing the dynamics of urban and regional development.

Among the vast quantity of literature on Berlin—ranging from broadest to most specific—this book lays justified claim as first to succeed Friedrich Leyden's major work, *Gross-Berlin: Geographie der Weltstadt* (1933). Its three major divisions are entitled "The Significance of Berlin's Location in the Course of Its History," "West Berlin's Population, Economy, and Movement System," and "The Texture of the Boroughs of West Berlin." The first fifty-eight pages of part 1 constitute an excellent summary of the physical development of

the city, including a recounting of the incorporation of *Gross-Berlin* in 1920 that doubled the city's population and multiplied its area by a factor of thirteen(!).

Historical development plays a major role in parts 2 and 3 as well, in which Burkhard Hofmeister analyzes the factors leading to West Berlin's current social and physical being. The inherent danger—that such a broad undertaking could easily become superficial—has been skillfully avoided in this book. It is a pleasant surprise to find that the solid statistical documentation creates a remarkably informative picture without overburdening the text. As a “structural analysis,” this work proves to be eminently readable scholarship.

Since there are to be only four other regional volumes in this series on Germany (plus one general summary and conclusion), it will be impossible for other cities to be treated with the admirable breadth and degree of detail that Hofmeister here has applied to Berlin. Historians of many specialties will find this to be a most rewarding study.

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JOHANNES VOLKER WAGNER, editor. *Der Parlamentarische Rat, 1948–1949: Akten und Protokolle*. Volume 1, *Vorgeschichte*. Boppard am Rhein: Harald Boldt Verlag. 1975. Pp. lxxxvii, 457.

The Basic Law of the Federal Republic of Germany, which has assured Bonn of a stable government through more than a quarter-century, has attracted increasing attention among American historians and political scientists. But the decisive negotiations between the Allied military governors and the heads of the West German state governments and the deliberations of the *Parlamentarische Rat* preceding the adoption of the Bonn constitution have not yet been analyzed adequately. The only notable exceptions are the monographs by John Ford Golay, *The Founding of the Federal Republic of Germany* (1958) and Peter H. Merkl, *The Origin of the West German Republic* (1963); they are supplemented by General Lucius Clay, *Decision in Germany* (1950). The German literature on the subject is somewhat richer, thanks partly to the memoirs of key figures like Konrad Adenauer and Reinhold Maier, to mention but two statesmen. The president of the Federal Archives, Hans Booms, the co-editor of the present work, however, correctly states in his preface that the accomplishments of the *Parlamentarische Rat* have rarely been evaluated and fully appreciated. He attributes this situation to the difficulties faced by scholars who sought access to the basic materials. The new series, jointly sponsored by the *Bundestag* and *Bundesarchiv*, will correct this shortcoming.

The present half-volume encompasses the most important documents on the prehistory of the *Parlamentarische Rat*. Johannes Volker Wagner, a director at the Federal Archives, has done an excellent editorial job. He has provided rich documentation for each piece, has written a lucid introduction of considerable length and assesses carefully the various steps leading to the establishment of the *Parlamentarische Rat* on September 1, 1948. The men who gave this preliminary parliamentary council its historic significance, foremost among them Konrad Adenauer, Theodor Heuss, and Carlo Schmid, appear here already on the scene. Wagner's essay is followed by twenty-eight documents. The most important among them are the *Frankfurter Dokumente* in which the three military governors of the West German zones present directives for the future political development of Germany; then comes the full text of the discussions at the conferences held by the West German minister-presidents and by some committees dealing with these documents. The second half-volume will contain studies of various important political bodies on major constitutional issues. Then the deliberations of the key committees of the *Parlamentarische Rat* will appear in full; they will occupy five volumes (plus an index volume).

This is a very ambitious undertaking. When completed, it will be an incomparable source for serious students of German political and constitutional history. Its editors are to be congratulated on the boldness of their planning and on the auspicious start of this monumental publication in the present volume.

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SÖREN WINGE. *Die Wirtschaftliche Aufbau-Vereinigung (WAV), 1945–53: Entwicklung und Politik einer „undoktrinären“ politischen Partei in der Bundesrepublik in der ersten Nachkriegszeit*. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 78.) Uppsala: Historiska Institutionen vid Uppsala Universitet; distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 251.

In this dissertation Sören Winge has struck upon historical riches. The story of the comet-like rise and fall of the post-World War II Bavarian “Economic Reconstruction Party” (WAV) and its colorful, demagogic leader Alfred Loritz yields not only much historical drama, but offers extraordinary insights into the unusual political situation of postwar, post-totalitarian Germany. The party thrived only in that brief period of roughly 1945–49 and combined within its program a whole spectrum of political demands and biases characteristic of the atmosphere of chaos, despair, and hope of those years. Winge has assembled the first full

account of this party and has been able to modify significantly the simplistic contemporary image of the WAV as a neo-Nazi gang without program. Strikingly young in its representation, urban in electoral base, and without a clear precursor, the party apparently followed its program with remarkable consistency despite unending leadership battles and the notorious instability of its parliamentary group.

Yet Winge's analysis remains disappointingly narrow and technical. His research base of printed parliamentary records, police documentation, and secondary works could have been profitably supplemented by valuable information available in unprinted committee minutes (especially *Wirtschaftsausschuss*); the private papers of Josef Müller, Josef Baumgartner, and Hans Ehard; the minutes of the Bavarian cabinet; and the *Institut für Zeitgeschichte* archive. One wonders whether Winge disregards Loritz's and others' alleged Nazi, espionage, and racketeering activities because the book is too narrowly focused on a technical analysis of the establishment, program, election statistics, and parliamentary practice of the party. More importantly, the WAV's historical significance as product and caricature of the extraordinary political reality of post-totalitarian Germany apparently escaped the author completely. Clearly the party's gut program of anticapitalism/antisocialism, small business and big production, antibureaucracy and state interventionism, and more democracy but less politics reflects the political power vacuum under occupation rule; it also mirrors the post-totalitarian blend of emotions in which the hatred of the totalitarian state and war merged with a distrust of the Weimar political-economic system responsible for the Depression and Nazism itself. Moreover, the nature of Loritz's power base dramatically exposes: 1.) a political system based on personal connections that ultimately rested on access to the occupying power, and 2.) the long shadow of the totalitarian experience, which kept a faith in the power of propaganda and the strong-willed leader alive. In this book Winge has as yet provided only the factual base for this kind of interpretation.

DIETHELM PROWE
Carleton College

NICHOLAS DER BAGDASARIAN. *The Austro-German Rapprochement, 1870-1879: From the Battle of Sedan to the Dual Alliance*. Cranbury, N.J.: Fairleigh Dickinson University Press. 1976. Pp. 334. \$18.00.

The Habsburg and Hohenzollern foreign policies associated with Julius Andrassy and Otto von Bismarck have generated some controversy, but histo-

rians differ more over the goals of Austrian diplomacy than over the aims of Germany policy. Nicholas der Bagdasarian does not challenge the traditional view that Bismarck sought to isolate France by diplomatic cooperation with Austria-Hungary and Russia; he merely seeks to modify the judgment that Andrassy's appearance at the Ballhausplatz marked a turning point in Austro-German relations and inevitably led to the Dual Alliance in 1879. After surveying the Franco-Prussian War, the formation of the Three Emperors' League and its diplomacy, the war scare in 1875, and Balkan affairs, he concludes that the rapprochement between Berlin and Vienna was more gradual than historians have suggested.

The policy of rapprochement was inaugurated in late 1870 by Andrassy's predecessor, Friedrich Ferdinand von Beust. Andrassy continued Beust's initiatives in Berlin within the framework of the Three Emperors' League. Yet the reconciliation between the two German courts did not eliminate mutual suspicions of each other's policies. For example Bagdasarian argues that the rumors of war in 1875 were intended by Bismarck as a warning to France and Austria to avoid any anti-German policy. The episode was exploited by Andrassy to strain relations between Berlin and St. Petersburg and led to "the next stage of the rapprochement—the culmination of a military alliance," (p. 181). The Austro-German defensive agreement was consummated four years later, but only after the Russo-Turkish War and the Congress of Berlin undermined the Three Emperors' League.

Since the Dual Alliance implied for Andrassy, though not for Bismarck, an end to cooperation with Russia, was this his goal since 1871? Bagdasarian says that it may well have been, although the Habsburg foreign minister felt constrained to attack the Russo-German friendship directly until success was assured (p. 24). He also argues that the "subsequent break with Russia was not a predetermined aspect of his policy, but rather resulted from . . . Austro-Russian rivalry in the Balkans" (p. 156). On the other hand, Bagdasarian indicates that the war scare in 1875 "marked another stage in the abandonment of [Andrassy's] previous policy of cooperation with Russia within the framework of the Three Emperors' League" (p. 180), while his Balkan policy from 1875 to 1877 reflected an earlier commitment to cooperation with St. Petersburg (p. 210).

What accounts for these divergent conclusions about Andrassy's diplomacy? The author has illuminated one aspect of the question by focussing on the changing nature of Austria's rapprochement with Germany. To clarify the problem further would require a more detailed examination of Austro-Russian relations and the role which Brit-

ain played in his diplomatic calculations—topics which go beyond the scope of this book.

JANIS ROGAINIS
Clemson University

RICHARD L. RUDOLPH. *Banking and Industrialization in Austria-Hungary: The Role of Banks in the Industrialization of the Czech Crownlands, 1873-1914*. New York: Cambridge University Press. 1976. Pp. xii, 291. \$28.00.

Some of the most basic issues regarding the pace, timing, and character of economic growth in the nineteenth-century Habsburg Monarchy are unresolved. Richard L. Rudolph has launched a direct attack on several of these questions in one of the most important books in Austrian history to appear in decades.

Rudolph's major thesis concerns the role of the Austrian banks in the process of industrialization. Many economic historians have argued that banks were active agents in economic development where they performed the entrepreneurial role in addition to normal banking functions. According to the Gerschenkron hypothesis on European industrialization it was especially in the relatively backward economies of Central Europe that the banks assumed this entrepreneurial task. Did the Austrian banks actually play this growth-inducing role? Rudolph's answer is an emphatic "No!" They were essentially conservative in outlook and displayed extreme caution in policy decisions. Rather than engage in the promotion of new firms they tended to cultivate existing relationships and attached themselves to "plump, juicy firms" (p. 104) only when they had already overcome initial growing pains. The Austrian banks, however, were neither an unimportant nor necessarily a negative force in economic development; as in Germany the bank-industry tie was both pervasive and intimate. Also, the banks in Austria performed functions, such as the selling of industrial output on commission, not widely performed elsewhere.

Rudolph adopts an analytical perspective and makes extensive use of statistics, which is refreshing in a field long dominated by narrative historians. To support his main thesis he relies heavily on evidence of a qualitative nature culled from bank and government archives in Vienna and Prague. Occasionally the imprecision which can accompany the use of this kind of evidence crops up. For example, it is unclear what specific criteria are used to judge the quality of entrepreneurship and the degree of conservatism in Austrian banking. But on the whole the thesis is soundly argued and well documented. Any attempt to apply a strictly cliometric approach to this subject matter is likely to reinforce and not alter the basic conclusions.

The most explicitly quantitative portion of the book involves the construction of an index of industrial production for the years 1880-1913. Specialists and nonspecialists alike will be indebted to the author for this contribution. The rate of growth shown by this index puts firmly to rest any notion that the Austrian economy was stagnating on the eve of World War I. And its rather steady upward trend indicates that the Gerschenkronian concept of the "great spurt" is not applicable to this Central European economy. Austria apparently achieved sustained industrialization without any identifiable discontinuity in industrial growth.

This book, unlike many specialized works, is not doomed to collect dust in library stacks. Nonspecialists will find it very useful as a tool for teaching and research. Chapters 1 and 2 summarize and expand our knowledge of the nineteenth-century Austrian economy. Students of banking history will profit from chapters 3 and 7 which outline the main characteristics of the Austrian financial structure and present the book's thesis. Above all, the author has consciously set out to relate his research on the specific case of Austria to larger issues in European economic history.

DAVID F. GOOD
Temple University

BERNARD MICHEL. *Banques et banquiers en Autriche au début du 20^e siècle*. (Cahiers de la fondation nationale des sciences politiques, number 199.) Paris: Presses de la fondation nationale des sciences politiques. 1976. Pp. 404.

Bernard Michel's study of Austrian banks and bankers is a treasure-trove of information about a part of Europe that is often pigeonholed into the category of "nationality problems." Big banks and bankers in Vienna and Prague from about 1900 to 1914 constitute the subject matter; their role in the economic development of Austria-Hungary lies at the core of the book.

Submitted in a larger edition as a thesis for the *doctorat d'État* in 1970, this weighty tome unrolls the practices of the banking houses of Cisleithian Austria in serial order. First comes the structure of the banks, headed by the official Bank of Austria-Hungary, followed by a number of other big banks (never enumerated or tabulated in full). The joint-stock investment banks never reached the heights of their counterparts in Germany, since savings banks and mutual credit societies remained strong down to World War I. Nevertheless, private banks and many provincial banks were absorbed by the Viennese and secondarily by the Germans and Czechs of Prague, all of whom gradually shifted their focus from state loans and mortgages to the financing of industry.

The bulk of the book deals with this transition from state loans to industry, and one receives an industry by industry account of what the banks in the two cities were doing, as well as some hard-to-find information about banks in some of the smaller cities of the empire. This story rarely transcends a narrative accounting, and the reader is left wondering on two essential points: 1.) How complete was the transition from mortgages to industry? That is, were the Austrians starting to follow the German route? and 2.) How important was the role of banks in promoting industrialization? (This latter analytic question stands at the center of Richard Rudolph's recently released book on banking in Austria-Hungary [1976]. It is remarkable that Michel and Rudolph find no place in each other's bibliographies.)

The relatively short section on bankers seems tacked on and is episodic and anecdotal in its treatment of the subject. There is much information on the bankers, the result of highly original research on both the life-styles of the banker group and their political influence (not great). Evaluation of the material remains for future work.

Michel stresses two themes which he claims to find at the center of the enterprise. The notion of German versus Czech banks reappears time and again, and the author states that his subject embodies the crucial dimension of Central Europe, that of multinational existence. From this point of view, he feels that any social category could have been chosen. This statement of belief hardly rises above the rhetorical. Czech nationalist appeals surfaced frequently in Czech banking practices in the last decade before World War I, but the thrust of the book suggests that national differences were only threatening to restructure banking patterns in the Dual Monarchy. The threat had not significantly materialized by 1914.

Secondly, Michel constantly measures the successes and failures of Austrian banking in the light of some overarching anti-capitalist, anti-banking atmosphere. Perhaps he is right, but conjuring up an image cannot substitute for a fully painted historical picture.

In sum, the author has ambitious analytic goals, but his success lies in the narrative plane, where he sets out an enormous amount of useful information about Austrian banks and Austrian economic development. Even here the contribution is marred by the lack of scholarly apparatus. It is incredible, even in these days of high printing costs, that a book of 405 pages, replete with direct quotations and brimming over with references to archives in Vienna and Prague, should be published without a single footnote.

LAWRENCE SCHOFER
University of Pennsylvania

GERHARD BOTZ. *Gewalt in der Politik: Attentate, Zusammenstöße, Putschversuche, Unruhen in Österreich 1918 bis 1934*. Munich: Wilhelm Fink Verlag. 1976. Pp. 358. DM 36.

That violence has been a major factor in twentieth-century political life is news to no one, least of all historians of Central Europe, where in the twenties and thirties city streets generally proved more attractive than parliaments as forums for political debate. But most commentators have remained content to point up the rather obvious correlation between the rise of political violence and the disintegration of parliamentary institutions. They leave one wanting to know more about the exact nature of that endemic violence: who, sociologically, were the participants? What were their motives? Which regions were most affected and why? In which of the contending camps does the major responsibility for the escalation of violence lie? In this monograph, Gerhard Botz poses just such questions about the history of the Austrian First Republic. This work is, quite simply, the most valuable study on political violence in Central Europe since Emil Julius Gumbel's *Verräter verfallender Feme* (1929).

Botz's book approaches the problem of political violence in three ways: first, it summarizes recent sociological and political-scientific contributions to the subject, and sets up the four conceptual categories (putsches, spontaneous riots, organized confrontations, and individual terror acts) through which the Austrian data will be organized; second, it provides a kind of blow-by-blow description of Austrian violence from the Communist putsch attempts of 1918-19 to the eve of the Civil War in February 1934; and finally, it subjects all the data to statistical analysis in an effort to reach some generalized conclusions about the Austrian case.

This tripartite approach proves useful and productive. The conceptual rigor allows Botz to investigate dozens of separate historical instances—from little-known lynchings or abortive assassination attempts to spectacular riots like the burning of the Palace of Justice in 1927—without getting lost in a maze of detail. At the same time, scrupulous attention to historical nuance prevents the author from succumbing to the opposite danger—that of constructing an elaborate theoretical skeleton unembellished by factual meat or muscle.

The final section in the narrative (a selection of documents follows the text) provides a clear and very suggestive summary of the data. Here Botz offers generalizations, supported by sophisticated statistical analysis, on the ages, occupations, political affiliations, and geographical origins of the "militants" in all contending camps. He also puts forth some qualified conjectures regarding the timing and motivation of the violence. We learn, *inter*

alia, that the most active participants were very young (16–20). Members of the “Marxist” camp were more likely to die or suffer severe wounds in violent confrontations than their rightist opponents. Students and commercial employees were “overrepresented” among the practitioners of rightist violence, but so were uneducated *Hilfsarbeiter*. The areas most prone to political unrest were the suburbs of Vienna, plus Graz and the mining districts of Styria. Protestants were more violent than Catholics. And finally, there was a positive correlation between unemployment and political violence. Terror of one kind bred terror of another: the Austrian First Republic was trapped in a vicious circle of destructive violence.

Botz’s study, for all its insights into the nature of political violence in general and its Austrian manifestation in particular, does contain one slight drawback. Focusing as it does on the sorry chronicle of killing, rioting, and looting, it leaves the impression that this self-destructive course was inevitable, that the First Republic was doomed from the outset to drown in its own gore. That a kind of vicious circle of violence *did* begin to operate cannot be doubted, but one should not forget that the moment of birth itself, the Austrian “revolution,” was relatively tranquil. There was initial potential for a much different, more peaceful evolution, a potential soon squandered by the country’s most prominent politicians. Unfortunately, politicians’ capacity for stupidity is not susceptible to statistical analysis.

DAVID LARGE
Smith College

PETER HUEMER. *Sektionschef Robert Hecht und die Zerstörung der Demokratie in Österreich: Eine historisch-politische Studie*. Munich: R. Oldenbourg Verlag. 1975. Pp. 372. DM 48.

This work was originally written as a doctoral dissertation under the supervision of Ludwig Jedlicka of the University of Vienna. Based on a large number of unpublished documents in the *Kriegsarchiv* and the *Allgemeines Verwaltungsarchiv*—as well as on published documents, newspaper articles, periodical literature, memoirs, and party records—it is one of the very best of the numerous first-rate studies written by former students of the Institute for Contemporary History at the University of Vienna.

To show how the bureaucracy assisted right-wing political groups in undermining democracy in Austria between 1920 and 1934, Peter Huemer focuses his attention on Robert Hecht, an able, intelligent, hard-working, and “correct” jurist, who headed the legal section of the army ministry and subsequently served as Chancellor Engelbert

Dollfuss’ key adviser on legal-political matters. During the 1920s, Huemer contends, antidemocratic tendencies were most clearly manifested by right-wing Christian Social politicians, conservative army officers, and reactionary officials in the army ministry, such as Minister Carl Vaugoin. These officials, aware that the workers would be helpless in a class conflict if they were deprived of all power and influence in the armed forces, steadily undermined the democratic spirit of the army, took away from the enlisted men the democratic rights guaranteed to them in the 1920 Army Law, and methodically recruited mainly conservative elements of the populace for the military units. By 1928 the Christian Socials had succeeded in turning the army into a reliable instrument of the bourgeois governing coalition.

In Huemer’s opinion, the struggle for the control of the army was only a preliminary round in the battle for the Republic. Hecht was at the center of both conflicts. After having ably assisted Vaugoin in undermining the military as a reliable supporter of the democratic republic, he became Dollfuss’ most trusted legal adviser. In this capacity Hecht provided him with many of the “legal, or more accurately, sham-legal” arguments (p. 137) to justify the numerous measures taken to destroy the democratic parliamentary government and to replace it with an “Austro-fascist” regime, as the author calls it. Among other things, Hecht first discovered the constitutional possibilities of resurrecting the 1917 war economic emergency decree to justify the systematic liquidation of democratic institutions, and he coined the term “self-elimination [*Selbstausschaltung*] of parliament” to defend the chancellor’s refusal to reconvene it.

Although the author calls the reader’s attention to the fact that the Social Democrats also made mistakes—mainly by being overly cautious and too reluctant to fight to defend their rights—the real villains are the bourgeois parties in general and, in particular, Vaugoin, Hecht, Dollfuss, and, above all, President Wilhelm Miklas. Huemer’s approach is that of a convinced democrat who essentially believes that a Social Democrat could do no wrong and that an opponent of the Republic could do no right. Even though his own political convictions may perhaps have occasionally colored his judgments, Huemer has written one of the most significant studies on the First Austrian Republic that has been published in the last couple of decades.

R. JOHN RATH
Rice University

FELIX PLATTER. *Tagebuch (Lebensbeschreibung)*, 1536–1567. Edited by VALENTIN LÖTSCHER. (Basler

Chroniken, number 10.) Basel: Schwabe Verlag. 1976. Pp. 579. 120 FR.

This volume closes the cycle of critical editions of the major writings of Basel's Platter family, which began in 1944 with the autobiography of the founding father and continued in 1968 with the travel diaries of his youngest son. Fitting between them, Felix's diary completes the collective portrait of an exceptionally self-conscious and successful group of sixteenth-century burghers.

Composed at age seventy-six from now-lost notebooks (his father had written an autobiography at seventy-two), these memoirs fall naturally into three parts. First come Felix's childhood years in Basel as the firstborn son of a selfmade man from the Alps with great expectations for his heir, until his departure for France at age sixteen. The middle part covers the four and a half years spent studying medicine at Montpellier—an extremely happy time, recalled vividly sixty years later, filled with boundless curiosity and enthusiasm. The final third covers the first decade of his adult life in Basel, when Felix returned home to take his doctorate in medicine and marry his childhood sweetheart—years devoted to establishing himself as a physician and setting up an independent household. This edition also includes later memoirs covering Felix's three trips to Germany (1577, 1596, and 1598) and his calculations of his total income from all sources between 1558 and 1612 (just over half of it came from practicing medicine; most of the rest came from rents and loans).

Felix Platter's diary—particularly his student years at Montpellier—has long been known to scholars: published versions date back to 1840, and there have been partial translations into French and English. This edition replaces all previous versions, however, and will probably not be replaced in the future; the editor spent more than a dozen years working on it, and the results are apparent. Copious footnotes, lavish illustrations, and two indices (by subject and by proper names) adorn this book, which is prefaced by a brief discussion of everything from Felix Platter's psychology to his dialect, paying special attention to his medical lore. This is a solid document to a native son, crafted with care and Swiss precision under the benevolent auspices of Basel's Historical and Antiquarian Society.

E. WILLIAM MONTER
Northwestern University

FREDY GRÖBLI. *Ambassador Du Luc und der Trücklibund von 1715: Französische Diplomatie und eidgenössisches Gleichgewicht in den letzten Jahren Ludwigs XIV.* In two volumes. (Basler Beiträge zur Geschichtswissenschaft, volumes 135, 135a). Basel: Helbing

and Lichtenhahn Verlag. 1975. Pp. xv, 331; 207. DM 72 the set.

Most historians of the period know that French intentions and actions were of overwhelming importance to the Swiss Confederation during the reign of Louis XIV. The content and significance of the *Trücklibund*—the name traditionally given to the secret agreement which the Sun King made with the Catholic cantons in May 1715—are not, however, widely known outside of Switzerland. It dealt with the recruitment of Swiss mercenaries and French support of the Catholics in their conflicts with the Protestant cantons, particularly Bern and Zurich. The terms of the *Trücklibund* and the way it was negotiated illustrate the stresses threatening the Swiss Confederation early in the eighteenth century.

Fredy Gröbli re-evaluates the personality and role of Charles-François de Vintimille, Comte Du Luc, during the eventful years from 1709 to 1715 when he was French ambassador in Switzerland. The interpretations of earlier scholars like Johann Casper Zellweger and Henry Mercier were inspired by patriotic zeal or literary ambition and thus were not objective. In his effort to be fair to Du Luc, Gröbli has made an exhaustive examination of printed materials and relevant archives; then, as is usual in dissertations, he carefully documents every point he makes (there are 148 pages of notes for the 331 pages of text). Although the ambassador's foibles and faults are clearly presented, the author's more positive evaluation of the man and his mission is quite convincing. Echoing the belief of Du Luc himself, Gröbli argues that the mission marked the beginning of a new era in Franco-Swiss relations.

In one sense this is a work of traditional diplomatic history. Every twist and turn of the negotiations, the plans and counter-plans of the numerous participants, all are presented in numbing detail. The minutiae almost cover up the significance of the story. Yet valuable information and ideas are scattered through the volumes. We learn about the difficulties of diplomatic negotiations in a country which was a collection of semi-independent units with no central authority capable of controlling affairs. This is a good example of how diplomatic sources can throw light on a country's internal history. The actual working methods of an early modern embassy are discussed, especially financial and personnel matters, and we learn about the problems involved in hiring mercenary soldiers. Gröbli shows that religious differences still had substantial diplomatic and political ramifications. Finally, the author's placing his study in the broader religious, diplomatic, and political spectrum of the early-modern period prevents the book from being just another diplomatic case study.

Scholars interested in a variety of topics will find value in this well-researched, well-printed, multifaceted book.

WILLIAM ROOSEN
Northern Arizona University

JEAN-CHARLES BIAUDET and MARIE-CLAUDE JEQUIER, editors. *Mémoires du Landamman Monod pour servir à l'histoire de la Suisse en 1815*. In three volumes. (Quellen zur schweizer Geschichte, New Series. Section 3: Abteilung Briefe und Denkwürdigkeiten, volume 9.) Bern: Selbstverlag der Allgemeinen Geschichtsforschenden Gesellschaft der Schweiz, Stadt- und Universitätsbibliothek. 1975. Pp. 717.

The restoration of 1815 involved in Switzerland a question of territorial rights. The prerevolutionary *Eidgenossenschaft* had consisted of sovereign cantons and a variety of subject districts. Six of the latter acquired a status of equality by the constitution of 1803, which was the work of Napoleon. Were they now to be restored to their former suzerains? Of all the questions that came before the "long diet" of 1814-15 this was easily the most divisive.

For the Swiss these years were not a time of glory. Internal affairs were dominated by men who had "learned nothing and forgotten everything." The Bernese, who led the reaction, were bent upon recovering their domain over Aargau and the Vaud. Of the several capable men Vaud sent to the diet the most eminent was Henri Monod (1753-1833), whose interesting memoirs of that period are now published for the first time. Monod was the canton's first *Landamman(n)*, or chief magistrate. He was a friend of Frédéric-César de La Harpe, who was tutor to Alexander I and a leading revolutionary. Monod shared his friend's enthusiasm for liberty but not his liking for extremes. At the diet Monod operated with great skill. While the reactionaries counted on the support of Metternich, the Vaudois banked on the goodwill of the tsar. Monod paid a visit to the Allied camp and spoke to Alexander, who gave (and kept) his promise that the new cantons should remain independent.

Monod's record deserves a wide reading. It is an agreeable book that has a general interest not lessened but rather heightened by its topicality. Opening a new view of Europe's condition in 1815, it also reveals a man of uncommon character who survived an age of difficult choices without moral loss. With Monod Burkean qualities were applied to a revolutionary goal. Monod recognized the force of general ideas (in other remembrances he credited the Enlightenment with having awakened the Vaudois to their true interests), but he shunned absolute doctrines and concentrated on the goal of Vaudois independence, achieving a lasting success.

The work presents a complete and verifiable history. Monod wrote from letters and other contemporary records. Though these were not attached to the manuscript, most have been gathered in the annexes. Jean-Charles Biaudet, who earlier co-edited Monod's *Souvenirs inédits*, has once more provided a topical history of general value.

HARRY F. YOUNG
Department of State

WILLIAM BARCLAY PARSONS. *Engineers and Engineering in the Renaissance*. With a new introduction by ROBERT S. WOODBURY. Reprint. Cambridge, Mass.: M.I.T. Press. 1976. Pp. xx, 661. \$8.95.

General Parson's massive study was left incomplete at his death in 1932; only in 1939 were his notes published in a small edition that became both rare and valued. Until the appearance of the five-volume *History of Technology* edited by Charles Singer and others (1954-58) and *Engineers of the Renaissance* by Bertrand Gille (1966, French edition 1964), this posthumous compilation served as the major review of its subject in English. And indeed its wealth of information has not been superseded by recent surveys and monographs.

Engineers and Engineering in the Renaissance is, despite its acknowledged riches, a difficult, cumbersome, and biased work. Where its title might imply sociology or social history, its content offers hagiography and measurements of buildings, bridges, tunnels, and canals. Though its size suggests comprehensiveness, the gaps are serious, with nothing for example on military engineering or surveying. As might have been expected of a general and a highly successful engineer—he surveyed for the Hankow-Canton railway, supervised construction of the New York and Chicago subway systems, and advised on the Panama Canal—Parsons was a positivist, who wrenched his "facts" from their social and historical context, and an elitist with a strong antilabor bias. The naive and exaggerated attention to facts, as independent entities requiring (or, harboring) no interpretation, yielded at least many useful tables, measurements, and minute technical descriptions. But for Robert Woodbury, in his introduction, to praise such outmoded historiography is tantamount to deception of student readers.

The M.I.T. Press editions of this book (hardcover 1968, paperback 1976) make significant deletions, thus tampering in a most unfortunate manner with the work's scholarly integrity. Omitting the first two chapters did not improve, or appreciably shorten, the book; it merely removed from view the more embarrassing expressions of Parsons' view of history. In his introduction Woodbury even claims: "It has the further merit of not attempting to derive any 'broad' significances; he

tells only the simple story of actual engineering development." Yet that Parsons did attempt more is evident in these comments from the portion censored from the reprint: "Perhaps the cause [of the end of the Roman Empire] will be found not in critical historical analysis but in the working of a law of nature. . . . In accordance with the laws of evolution and of development the end had come. . . . For ten centuries human energy was to lie practically dormant. . . . The wresting of the Magna Charta from King John and the defeat of the Saracens at Tours by Charles Martel . . . are the best of the few glories that illuminated the Middle Ages."

Allowing for such conceptual defects, considerable value remains in this unfinished work. It offers over two hundred worthy illustrations, generally well explained in the text; long quotations in English from primary sources; an engineer's appreciation and analysis of Leonardo da Vinci; and detailed accounts of specific feats of engineering such as canal construction, several important bridges, and two justly famous domes. Within the limits of his abilities and the conditions of his personal experiences, Parsons achieved a remarkable survey. His ideas and his book, despite recognized structural weaknesses, deserve criticism rather than cover-up.

BERT HANSEN
State University of New York,
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AGOSTINO RAMELLI. *The Various and Ingenious Machines of Agostino Ramelli* (1588). Translated by MARTHA TEACH GNUDI. Annotations and a pictorial glossary by EUGENE S. FERGUSON. Baltimore: Johns Hopkins University Press, 1976. Pp. 604. \$100.00

All historians know that Italian engineers developed the Renaissance art of fortification in their city wars and taught it to the rest of Europe; it is perhaps less obvious that some of these engineers were also able mechanics. One of them was Agostino Ramelli (ca. 1531–post-1608), who entered the service of the Duc d'Anjou (later Henri III) and was under his command at the siege of La Rochelle in 1572. His book, *Le diverse et artificiose machine* (1588), dedicated to this monarch, is very familiar to historians of technology; a reduced facsimile of it was published in London in 1970. Consisting of 193 pictures of mechanical devices, with accompanying short descriptions in both Italian and French (the two do not always perfectly concur), it is unlike Lorini's *Le Fortificationi*—which, especially in its larger third edition of 1609, fits into the same tradition of book-making—in hardly relating to its author's profession: while thirty-nine devices have military applications, over a hundred are "pumps" of varied sorts and twenty-five are mills.

Fortification in the ordinary sense is not considered.

Martha Teach Gnudi has considerably amplified the thin narrative of Ramelli's life, but many historical questions are necessarily left unanswered in this opulent volume. No sustained attempt is made to fit the book into the wider context of technological literature and technological practice extending from Valturio to Böckler. Although the chapter notes (general comments on the pictures) and pictorial glossary (again, with notes) do in part fulfill this function, a study of the literature as a whole remains a desideratum. (Piecemeal, a good deal has been done in recent years). Eugene S. Ferguson devotes several pages to the question of "Precedents and Other Influences," concluding, surprisingly, and in opposition to the well-documented 1972 article by Ladislao Reti, that no "borrowing" by Ramelli from the designs of Leonardo da Vinci can be proven. While Reti exaggerated the ascertainable importance of Leonardo in the engineering tradition, it stretches credulity to assign the correspondences between half a dozen or so of Ramelli's ideas and those of Leonardo to coincidence or common ancestry. To argue, as Ferguson rightly does that Ramelli's plates are superior in many details to Leonardo's rough sketches does not meet the point that a very imperfect sketch may inspire a highly finished design. Somewhat similarly, Ferguson's assessment of the influence of Ramelli's book, in general just, does not consider the question: how much of all this inventiveness was practical and useful, and how much for show?

In the last resort the value of a translation of a Renaissance technical text must depend upon its combining accuracy with intelligibility to a modern reader. Gnudi's English versions of the descriptions of each device are indeed generally intelligible, and a modern reader who studies each plate carefully will perhaps never be misled as to its function and action. But it is strange that so experienced a translator quite fails to reproduce the ornate style of the original dedication and preface, and can turn "*tenebreuses chauves-souris*" into "brown owls." In technical language more apposite terms than those chosen suggest themselves: "channel" rather than "canal," " housings" of pumps rather than "cases," "pieces" rather than "strips" of leather in a piston, etc. Then there are a number of clear mistranslations. The repeated description of a pair of wheels as "*dentées l'une au contraire de l'autre*" means "toothed in opposite segments" not "toothed on facing sides."

Upon such details the student must still consult the original text; nevertheless in general the volume does ample justice to the historical importance and esthetic attractions of Ramelli's book and by its editorial contributions provides new

guidance for the student of Renaissance technology.

RUPERT HALL

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PIERO DEL NEGRO. *Il mito americano nella Venezia del settecento*. (Atti della Accademia Nazionale dei Lincei—1975, Memorie, Classe di Scienze Morali, Storiche e Filologiche. Series 8, number 18, part 6.) Rome: Accademia Nazionale dei Lincei. 1975. Pp. 447–656.

Piero del Negro has given us a compact and informative study of the American mythos in the Venetian culture of the *settecento*. His work is a felicitous balance of extensive documentation and clarity of expression.

Venice in the *settecento* was approaching extinction as an independent political entity, but its intellectual and cultural life, like that of all Italy, was entering a renaissance *giovanezza*. After centuries of strife Italy enjoyed a few decades of peace before being overwhelmed once again by the turbulence of European war. But a few decades were sufficient for the Italians to create a new Renaissance within the context of the European Enlightenment.

The intelligentsia of Italy looked to America for the realization of reform and revolution. Gaetano Filangieri, the leading light of the Neapolitan reformers, corresponded with Benjamin Franklin, and considered settling in Philadelphia. Filippo Mazzei, the Tuscan revolutionary, did much to forward the cause of independence in Virginia. Vittorio Alfieri wrote his *America the Free* in tribute to American patriots.

Del Negro discusses the *cinquecento* and *seicento* images of America in Venetian thought, but he finds that it was only in the *settecento* that the singular reality of the American experience received significant attention from Venetian belletrists. He refers to the iconography of the Americas in Tiepolo, Pellegrini, and Crosato. Algarotti followed the conflict between the French and the British in North America. Goldoni wrote *La Peruviana* and *La bella selvaggia*, the latter being another example of the Enlightenment's conception of the natural morality and primitive wisdom of New World aborigines.

Del Negro dwells on Raynal's influence in the Veneto. The *Histoire des deux Indes*, a best-selling hodgepodge of fact, opinion, and misinterpretation, went through fifty-four European editions; it was superficial and sometimes unhistorical, but its influence should not be underestimated. Even Filangieri, perceptive critic that he was, repeatedly cited Raynal in the *Scienza della legislazione*. Raynal's deistic bias stung the Venetian clergy, especially *padre* Antonio Valsecchi

who, in his *Religione vincitrice* (1776), deplored Raynal's "poisonous" ideas. In 1777 Raynal's work was proscribed.

There was little reason for Venice to involve herself in the American Revolution, but the British defeat was greeted with satisfaction and the prediction that a vast American empire would dominate the West. Del Negro cites Carlo Palese's view that oligarchic despotism had fallen before democracy and that the ambition of the few had been stifled by the general will.

Del Negro provides a wealth of information and perceptive insight. His work is a welcome contribution to the study of the circulation of ideas in the most cosmopolitan of centuries; it merits translation into English for the benefit of a larger readership.

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GIORGIO SPINI *et al.*, editors. *Italia e America dal settecento all'età dell'imperialismo*. (Nordamericana, number 1.) Padua: Marsilio Editori. 1976. Pp. 472. L. 9,000.

GIORGIO SPINI *et al.*, editors. *Italia e America dalla grande guerra a oggi*. (Nordamericana, number 2.) Padua: Marsilio Editori. 1976. Pp. 283. L. 5,500.

These twin volumes on Italo-American relations belong with the huge collection of courtesy works inspired by the bicentennial of the United States. They could and should have been far more. The editor-in-chief is veteran historian Giorgio Spini of the University of Florence, at one time or other visiting professor at Harvard, Berkeley, and the University of Wisconsin. Twenty-seven of the thirty contributors are associated with leading Italian universities, and most of these writers have studied or visited in the United States.

The stated purpose of *Italia e America* is to study the *impact* of American history on Italy. This aim should not have been impossible to fulfill. Four themes, among others, might have been elaborated: the force of the American myth on Italian aims and ambitions; the example of separation of Church and State in the United States which led to repeated questioning by the Italian government, through diplomatic channels, in the hope of a satisfactory arrangement with the Vatican; the effect of Italian emigration to the United States and the monetary gain to Italy, as well as the social repercussions, of easy repatriation; the whole gamut of Italo-American economic relations. Of solid points such as these, only the myth of the United States is considered at length, and even this is treated only indirectly in such essays as "Italian Illuminism and the American Revolution," or "The Image of

the United States" in this or that press. On the other hand, one can find in these books very interesting essays on particular aspects of history involving Italians and Americans. An example in point is Maddalena Tirabassi's "The Mazzini Society (1940-1946): An Association of Italian Anti-Fascists in the United States."

The monographs are all carefully researched, well documented, and chronologically arranged. Volume one is divided into three parts: The Age of the American Revolution; From the Formation of the United States to the Age of Reconstruction; and the Age of Imperialism. Volume two has two main divisions: From the Great War to the Second World War; and Postwar. Each main section is preceded by an informative introduction; each book has a long invitational by Spini. Together, these prefatory remarks by various authors are a valuable summary of aspects of Italian-American interaction. It is here that one learns that the American myth was the dream only of Italian radicals, while the establishment in Italy disdained the United States. Here we find that, despite the romanticizing of Garibaldi, the Italians generally regarded our Civil War as a trifle. We learn, too, that Italian liberal economists made a completely negative judgment of the New Deal. All of these interesting points, however, are mere suggestions of a far larger picture accentuated randomly by the substantive monographs. The problem is that the main essays are discrete, individual research projects linked only loosely by a time chain. A generalizing, correlating theme is missing.

Perhaps more than anything else, these books point up the need for a massive, thorough study of Italian-American relations, a task for which a great amount of spadework has already been done by authors like Leo F. Stock, Howard R. Marraro, Kent Roberts Greenfield, Charles F. Delzell, and A. M. Ghisalberti. For that definitive work of the future, *Italia e America* will furnish indispensable preparatory studies. Indeed, Spini ends his preface to volume two with the express wish for a "vast and profound study of the history of relations between Italy and America, stimulated by this collection of essays" (p. 19). Here is a lifetime project for some young scholar.

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FRANCO VENTURI. *Settecento riformatore*. Volume 2, *La chiesa e la repubblica dentro i loro limiti, 1758-1774*. (Biblioteca di cultura storica, number 103.) Turin: Giulio Einaudi, 1976. Pp. xvii, 355. L. 10,000.

Those historians of the Enlightenment and of modern Italy who profited from Franco Venturi's

Da Muratori a Beccaria (1969) will find different but no less satisfying fare in this second volume of his projected trilogy on Italy's *Settecento riformatore*. While the first volume focused upon the intellectual contributions of men committed to a renewal of Italian culture, this one explores the political and social implications of those contributions.

The subtitle ("Church and Republic within [their respective] limits") directs the reader's attention to what Venturi perceives as the most important issue of the 1750s and 1760s in Italy. In the land of the triumphant Counter Reformation, he argues, even modest attempts by reform-minded statesmen to change political and judicial institutions, to stimulate economic growth, or to improve the training of the country's future leaders resulted in confrontations between Church and State. Above all, the reformers' attempt to define "limits," separate spheres for the exercise of religious and of secular authority, was more bitterly contested in Italy than anywhere else in Europe.

The literary and statistical evidence used by Venturi point to a country in which the prestige of countless religious orders was still high at mid-eighteenth century and in which the practice of *mainmorte* limited the amount of land and other assets available for the development of a capitalist economy. Venturi discusses the clergy's argument that the accumulation of wealth in their hands was necessary to their charitable mission, but his sympathies are clearly with those bold spirits who observed that the poor needed employment, not charity.

If the Italian reformers had a difficult task, they did not feel isolated. On the contrary, they were spurred to action by the example of their counterparts in Spain and Portugal and by the anti-Jesuit currents in France. Having outlined the position of the clergy in Italian life and having sketched the international network of which the Italian reformers were a part, Venturi examines the confrontation between Church and Republic in the specific context of each Italian state. His purpose is obviously to seek out those problems that were common to all of them. Without glossing over significant differences, he finds that in every part of Italy the reformers aimed to reduce the number of religious orders, to secularize the education of young nobles, and to curb the growth of *mainmorte*; and he argues that by the 1760s a nationwide movement had emerged. As evidence of this he cites not only the correspondence among the reformers, but also the publication of *La Chiesa e la Repubblica dentro i loro limiti*, attributed to the Tuscan Cosimo Amidei, and of Carantonio Pilati's *Di una riforma d'Italia*.

If the first chapters of Venturi's book, which deal

with the impact of European events upon the Italian reformers, are a must for historians of the Enlightenment, the chapters on Amidei and Pilati will make exciting reading for historians of Italy's *Risorgimento*. The struggles of famous and obscure reformers, Amidei's search for a separation of religious and secular authority, and Pilati's insistence that the secularization of Italian society was a *sine qua non* for political and economic progress should lay to rest the notion that the radical anticlericalism espoused in the 1850s by men like Cattaneo, Pisacane, and Ferrari was based on irrelevant foreign models or on a misreading of Italy's intellectual traditions.

Venturi's book represents the best to date in the sparse literature on eighteenth-century Italy. Along with Carlo Dionisotti's seminal work, it deserves an English translation that would make it available to a large scholarly audience.

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ALFREDO GRANDI, editor. *Processi politici del Senato Lombardo-Veneto, 1815-1851*. (Istituto per la storia del Risorgimento italiano, Biblioteca scientifica. Series 2: Fonti, number 67.) Rome: the Institute. 1976. Pp. xxxviii, 779.

This work presents documents of the Lombard-Venetian kingdom's highest court. Most of the surviving items (those not destroyed during the Second World War) concern political cases which were handled by the court's criminal affairs division. The first section contains communications from the emperor, many of which are unavailable in other sources. The editor has also included a collection of governmental acts for the years 1848-51, as a substitute for documents lost during the revolution of 1848 and its aftermath. Statistics on crimes and their judicial disposition are also provided. Although this collection focuses on political trials, it incidentally provides much valuable information on general penal administration. The excellent index and Alfredo Grandi's introduction, which describes the judicial structure and procedures, are valuable aids.

The numerous political trials documented in this volume testify to the growth of opposition to Austrian rule. The emperor's messages, which frequently express concern about possible laxity in the handling of treason cases and the problems presented by Young Italy ("more dangerous than the Carbonari") reveal the Habsburg monarchy's frustration over the rise of nationalism. These records, moreover, reflect the growing participation in anti-Austrian activity by persons of good social standing—professional people, nobility, clergy.

Prominent *Risorgimento* names, such as those of Balbo, Cattaneo, and Gioberti, abound. Also interesting are the efforts of the Austrian authorities to obtain a reciprocal extradition treaty with Piedmont, increasingly a refuge for the Habsburg monarchy's dissident Italian subjects. In short, these previously unpublished, indeed almost unknown, documents are a vital source for a more complete appreciation of the events and issues of the *Risorgimento*.

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PIERO PIERI and GIORGIO ROCHAT. *Pietro Badoglio*. (La vita sociale della nuova Italia, number 22.) Turin: L'Unione Tipografico-Editrice Torinese. 1974. Pp. 914. L. 14,000.

GIOVANNI DE LUNA. *Badoglio: Un militare al potere*. (Information storica Bompiani.) Milan: Casa Editrice V. Bompiani. 1974. Pp. 284. L. 2,000.

Two biographies of the controversial and important Italian military and political figure, Marshal Pietro Badoglio (1871-1956), appeared in 1974, one brief, the other quite long. One was written by Giovanni De Luna of the University of Turin. Subtitled "A military man in power," his sprightly account focuses primarily on the period from the 1920s to 1944. The longer, and heavily annotated, study is the product of two distinguished scholars in the field of Italian military history: Piero Pieri, emeritus professor at the University of Turin, and Giorgio Rochat, professor at the University of Milan. Pieri is especially well known for his military histories of the nineteenth century, while Rochat is the author of pioneering studies of Italian military history during the Fascist era. Their excellent book appears in an impressive new biographical series on the post-*Risorgimento*, coordinated by Nino Valeri. The volume contains a very helpful 32-page essay on the sources used and a discussion of the kinds of archival materials that the Italian Army still withholds from outside historians. The authors have included numerous photographs, maps, and an appendix that summarizes the high points in Badoglio's career.

Pieri is the author of part 1, which covers Badoglio's youth in Piedmont and his participation in the Libyan conflict and the Great War. He pays special attention to Badoglio's role in the Caporetto debacle. He is judicious in his assessments of blame. Rochat has taken responsibility for part 2, covering the post-1919 era. He is also balanced in his criticisms of Badoglio's military and political actions, which became even more controversial in these years.

Appointed to the Senate by the king in 1919, Badoglio was always primarily loyal to the 1959

House of Savoy. Yet he also reached a certain accommodation with the Fascist regime. From 1923 to 1925 he was ambassador to Brazil. Upon his return to Italy, he was named Army Chief of Staff, a post he held until he fell from Mussolini's favor during the disastrous Greek campaign (December 1940). Those years were filled with almost constant intrigue and jealousy between Fascist hierarchs and the army leadership. Badoglio was promoted to Marshal of Italy in 1926 and appointed governor of Tripolitania and Cyrenaica from 1928 to 1933. His role in the Ethiopian War (1935-36) marked the high point of his military career.

Badoglio obtained his revenge on Mussolini on July 25, 1943, when King Victor Emmanuel III dismissed the Duce and asked the army veteran to join him in a royal-military dictatorship. The two old men bungled the armistice negotiations with the Allies. They were obliged to flee ingloriously from Rome on the night they announced the armistice (September 8/9, 1943), lest they be captured by the Germans. Taking haven with the Allies in the South, Badoglio and the king were propped up by Churchill, who was especially determined to hang on to this Italian government to insure continued fulfillment of the armistice terms. But none of the new political parties in the Committee of National Liberation would join Badoglio's controversial government. The deadlock was not broken until April 1944, when the returning Communist leader, Palmiro Togliatti, suddenly reversed his party's line and indicated his readiness to join Badoglio and the king for the purpose of strengthening the war effort. Badoglio's new political government was short lived, however. As soon as Rome was liberated, the anti-Fascist leaders managed to unseat him, much to Churchill's dismay. Embittered, Badoglio spent some of the last years of his life writing his controversial memoirs and blaming others for failure to defend Rome against the Germans in September 1943, and defending himself against unfair efforts to punish him for his past links with the Fascist regime.

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GUIDO QUAZZA. *Resistenza e storia d'Italia: Problemi e ipotesi di ricerca*. (Biblioteca di Storia Contemporanea, Testi e saggi, number 9.) Milan: Feltrinelli Editore. 1976. Pp. 468. L. 5,800.

Guido Quazza's book is at once a presentation of recent research from the Turin school of Marxist historiography and an original interpretation of the Resistance as part of the general history of Italy from 1922 to 1946. In Quazza's view, the

Resistance should be seen, not as a failed revolution, but rather as a step in the moral and political education of Italy. He feels that in order to understand why the Resistance represented no radical departure the conduct of the three major forces behind the opposition to Mussolini must be examined: the partisan bands of young peasants and workers, who had never accepted the Fascist regime's propaganda (Quazza totally rejects the recent interpretation, put forward by Renzo De Felice, on the extent of Fascist consensus); the political parties, which were represented in the Committee of National Liberation (CLN); and the "Fascist Anti-Fascists" (the Church, army, industrialists), who broke with the regime at the last moment.

In the struggle over post-Fascist Italy, the conservatives, backed by the Anglo-Americans, won almost by default. Quazza analyzes the difficulties in converting the partisan experience into political terms. The partisan movement was too involved in the struggle on the local level against the Germans and their Italian collaborators to offer a revolutionary alternative. The CLN, which aspired momentarily to a Jacobin-style government, found itself caught between two conflicting strategies in 1944. The first, proposed by the Action Party and some socialists, envisaged using the CLN as an instrument to forge new unity among leftist forces. The second, formulated by Palmiro Togliatti, viewed the chances of a radical break with the past pessimistically and opted for the long-range strategy of creating a solid base in the Communist Party. Quazza differs from other interpretations of Togliatti's policies by emphasizing the domestic considerations behind them. The Communist leader's acceptance of the royalist government in April 1944, against the wishes of the other Left parties was viewed not merely as adherence to a policy laid down in Moscow, but as a vital step in the transformation of the Italian Communist Party from a small clandestine force to a mass-based legal organization.

My only objection to Quazza's effort to put the Resistance in the broadest possible context is his dismissal of intellectual dissent within Fascism and the ability of Fascist leaders to create links with free culture as factors in the continuity between the *ventennio* and post-Fascism. While not properly part of the Resistance, the enormous number of people who made their careers under Fascism and could not totally repudiate it offered a powerful constituency against radical change. In all, however, Quazza has written an important work which presents much recent research into little studied areas of Resistance history.

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OTA ŠIK. *Das kommunistische Machtsystem*. Hamburg: Hoffmann and Campe. 1976. Pp. 357. DM 26.

The field of Communist studies seems to be at a standstill today. There are no more gimmicks to latch onto for the multitude of social scientists who have ridden every wave of new fads with an easy conscience, a new-found intellectual enthusiasm and conviction, and an ever-longer *curriculum vitae*. Admittedly, the road to the present doldrums has been long, interesting, and intellectually stimulating; even if one considers only the "seminal" works of the last thirty years dealing with the study of Communist political systems, the more than three hundred volumes published in English alone(!) would present something of a heroic task to master. And that list would not account for the literally thousands of books written in other languages, or the myriad words contained in articles and notes or in papers read at conferences. It is safe to say that during the last thirty years since its emergence in the aftermath of World War II, the field of "comparative Communist politics" has certainly come of age, or at least it has aged, along with most of its practitioners.

It all began in the late 1940s, of course, with those who were trained in law comparing the types of codes and laws that were evolving under Communist tutelage in Eastern Europe and in the USSR. Such volumes as Samuel L. Sharp's *New Constitutions of the Soviet Sphere* tended to focus on parallel elements of the legal structure and explain the all-too-common behavior of the East European states on this basis.

The "ideologists" for whom the behavior of the Communist states was explainable by the immutability of dogma began to replace the "legalists" early in the 1950s. Led by such intellectuals as Bertram Wolfe, R. N. Carew-Hunt, Carl J. Friedrich, Hannah Arendt, *et al.*, a new school of students of politics soon emerged, who believed that the key in analyzing Soviet and Communist society lay in the type of totalitarian rule that the party administered in all of the Communist states.

The slow alteration of Stalinist rule brought even the most rabid "totalitarianists" to a halt, left them baffled, and forced them to shift gears. A new school, the school of comparative Communist politics, began to make its appearance in the mid-1960s, with many of its members bringing well-founded generalizations with them and applying their own standards and formulae for instant wisdom. One learned that Communist political systems can "only" be understood as a developing model, as a process of increasing interest-group behavior, or as a bureaucratic instrument, and these terms did seem to bring a new dimension to our research.

And then in the mid-1960s, behavioralism hit the discipline, and created a new breed of analyst confident of his truth and his convictions. The behavioralist, following the "mainstream" of American political science and accepting the rather biased and dubious claim that one can measure behavior empirically, heroically attempted to examine the Communist systems with tools borrowed from students of American politics. Never mind that the method employed in studying macropolitics in the United States could not be replicated in the USSR and Eastern Europe; we took our data-base from wherever possible. Someone studied the elites in the United States? Well, let's study them in Eastern Europe. Data on the Central Committees of the Communist Parties to some extent were available from sources like the archives of Radio Free Europe. Never mind that the data were incomplete, that the weight of the Central Committees shifted with time, that some parts of the ruling elites were frequently outside of that nominal body, that many of the policy leaders were non-members; the data had to be milked and utilized. And if the data were not to be had, smaller and smaller units of analysis were used to make greater and greater conclusions. Instead of learning the languages, histories, and cultures of the regions, we occupied ourselves and our students with the teaching of techniques and methods and models.

Despite all my criticism, I too have been struck by the force of prevailing winds—the discipline as a whole has clearly benefited from all these approaches. Maturity, depth, and competence come very hard to any new discipline; intellectual terms and concepts have to be borrowed; tools have to be adopted; and others whose views, methodologies, and conclusions happen to be different from one's own must be looked upon not with condescension or contempt but with respect for the attempt that all make at illuminating new areas. As the discipline thus matured, as its practitioners aged, and as younger persons have come to an already flooded marketplace, the singleminded dominance of fads disappeared, students began to learn anew the languages, literatures, and cultures of the systems they were studying, and they acquired mechanical tools as well to use wherever each appeared to be appropriate. The studies published during the last two to three years have all been more modest in their scope and claims and hence more "humane" in what they were really trying to accomplish.

The publication of Ota Šik's volume falls into the gap between the earlier age of grandiose analyses that still prevails in Europe and the more modest American school. It claims to describe the entire system of power in the politics of all

Communist states of Europe, in spite of the fact that there are only a very few references to states other than the USSR and Šik's native Czechoslovakia. In spite of this limitation, his book is an enormously interesting and often brilliant exposé of the bureaucratized Soviet type of Communist system. The author, an economist by training and a deputy prime minister of Czechoslovakia during the Prague Spring of 1968, is now a professor in the College of St. Gallen, and the volume he has written reflects the sentiment and even naiveté of the Dubček government.

In this coherent survey Šik's main thesis is that the system in existence in the Communist states of Europe is a bureaucratic system of power, where the party-bureaucracy jealously guards its privileges and its primacy and perverts an otherwise healthy ideology for its own ends. With frequent reference to Max Weber's classic theories, after a brief analysis of the development of the bureaucratic Soviet state, in such chapters as the "Personal Power Interests of the Functionary" and the "Specific Structure of the Bureaucracy in the Communist System," Šik ties the existence and structure of the system to the self-perpetuating mechanism of the bureaucracy. He correctly identifies the sub-components of the mechanism of the Communist system of power as the theory and practice of the leading role of the party, cadre politics, the existing tools of repression and corruption, and the ideological monopoly of the party. He lashes out in anger against the bureaucratic antidemocracy that pervades the system and characterizes the bureaucratic process of production, the exploitation of the workers, the alienation of men, and the state's monopoly as the results of perverting the "true socialist system."

Šik's book will be a troublesome volume for many, and there are several reasons for the unease with which one may read the book. First and foremost it is a Marxist book, with the tendentious, difficult, and often incomprehensible logic of Marxism interposed with non-dialectical reasoning. Second, unlike most American social scientists and many of his own colleagues and compatriots, Šik remains a convinced believer that "socialism" without the bureaucracy is still the best possible system. And yet if one were to take away the culprits he accuses of perverting the system—the overwhelmingly bureaucratized *apparat*, the party that plays a leading role in the state, the cadre politics, the repressive elements of the system, the ideological monopoly of Communism, the state's monopoly over all facets of life—what type of system would really be left? Surely, not a "socialist" one as defined by Brezhnev, Tito, or even Kádár, and perhaps not one so defined by Golda Meier, Olaf Palme, Harold Wilson, or Willy Brandt. It

would indeed be a socialism with a human face, a system that could conceivably exist in a rich continent, not threatened by economic or political turmoil, by class divisions, by war, by nationality problems—in short a state that is not likely ever to exist in Eastern Europe. Perhaps it is too much to ask of a man who invested nearly forty years of his life in a belief in the perfectability of man to admit that the dream he searched for so relentlessly has turned into an ever-present nightmare. Perhaps it is enough for us to realize that in this volume he consciously updates his *spiritus rector* Max Weber's conclusions that in the Communist state "it is the dictatorship of the bureaucracy and not the dictatorship of the proletariat that is advancing." Perhaps we should be content with his conclusion that some fifty years after Weber's statement "... the existing bureaucratic system that developed can not be regarded as a childhood disease of socialism, that will disappear with time. . . ." We should simply admire this faith "in a democratic and human socialism" (p. 272) and not ask Šik to explain how that can ever come about.

Šik and many other leaders of failed revolutions who strove to attain noble goals, wander the landscape among us who have only studied the system of Communist rule. His is an uncomfortable presence here for he reminds us of moral values, moral failures, and moral commitments, and with these orientations the discipline of Communist studies has rarely been able to grapple in an adequate manner.

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LUCIAN BOIA. *Evoluția istoriografiei române* [Evolution of Romanian Historiography]. Bucharest: Universitatea din București, Facultatea de istorie. 1976. Pp. 377. 16.25 L.

Until now, interestingly enough, there has been no comprehensive study of the development of Romanian historical science. The work by Gheorghe Panu, *Studiul istoriei la români* (1873–74), the first general survey of the subject, or the classic inquiry by Ioan Bogdan, *Istoriografia română și probleme ei actuale* (1905), or the host of specialized studies since then on selected aspects and periods of Romanian historiography have, to be sure, provided valuable guidance to a complex evolution. Lucian Boia's work attempts for the first time to provide an overall narrative account of Romanian historiography from the fifteenth century to 1944. Intended primarily as a text for university students, it is straightforward in presentation and scholarly in evaluation.

The author divides his study chronologically into two main sections: medieval, from the earliest

chronicles in Slavic to the works of the first true historians—Constantin Cantacuzino and Dimitrie Cantemir—of the end of the seventeenth century; and modern, commencing with the works of the Transylvanian School and ending with Marxist historiography of the period between the two World Wars. In describing the principal currents and the individual authors and their works, Boia displays a sure command of his subject and assumes a refreshingly critical attitude toward controversial questions and the classics. Though he agrees that Cantemir's *History of the Ottoman Empire* (1716) brought him deserved European fame, he argues that his *Hronicul romano-moldo-vlahilor* (1719–22) is of greater historiographical importance in that it provides a link between the old Romanian historiography and the new. Boia draws interesting distinctions between the historiography of the Enlightenment in Transylvania and that in the Romanian principalities, pointing out that the latter was more developed owing to the mixing of two cultures and languages—Greek and Romanian. He offers an objective assessment of the strengths and weaknesses of Nicolae Bălcescu's works, judging his studies of social and economic history to be more significant than the better known *Istoria Românilor sub Mihai Voda Viteazul* (unfinished at the author's death in 1852). His characterization of Junimea as a reaction to romanticism and of the role of Titu Maiorescu is balanced; so is his portrait of Alexandru D. Xenopol, who is seen as lacking the originality and imagination of a Bogdan Petriceicu Hasdeu or a Nicolae Iorga, but nonetheless as possessing a spirit of organization and synthesis second to none. Commendable also are the sections on Ioan Bogdan as the outstanding representative of the "Critical School" at the turn of the nineteenth century, and on Iorga, whose long and varied career is compressed into thirty pages and whose strivings toward greater and greater synthesis form a leitmotif throughout his enormous productivity. It is also good to see recognition accorded the historians of civilization like Ștefan Zeletin and Eugen Lovinescu, and the historians of the national idea like the Uniate Augustin Bunea and the Bukovinian Ion Nistor. On Marxist historiography, the analysis of the writings of Constantin Dobrogeanu-Gherea and Lucrețiu Pătrășcanu is sober and critical.

Boia is careful to place Romanian historiography in a European setting, since it was to a greater or lesser extent always a part of broader movements. He also notes the contributions of Romanian historians to the elucidation of general problems of European history, like Andrei Oțetea's *Renășterea și Reforma* (1941), an interpretation of the Renaissance and Reformation based upon

social and economic data, and Gheorghe Brătianu's plan (in 1938) for a three-volume *Nouvelle histoire de l'Europe au Moyen Âge*, intended to bring out the essential (and neglected) role played by Eastern Europe in the evolution of the whole continent. Nonetheless, Boia concludes that the essential characteristic of Romanian historiography throughout its evolution has been its concentration upon the national destiny as exemplified in its themes (the unity and independence of the Romanians), its general conception of history (human progress takes place mainly through peoples and nations), and its emphasis upon the social mission of the historian (history can never be solely an intellectual preoccupation).

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STEVEN BELA VARDY. *Modern Hungarian Historiography*. (East European Monographs, number 17.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. xii, 333. \$20.60.

The aim of Steven Bela Vardy's study is "to present a comprehensive view of the development of Hungarian historical sciences from the eleventh to the middle of the twentieth century" (p. vii). His emphasis, however, is on the twentieth century, with earlier periods summarized in encyclopedic rather than analytic fashion. This summary amounts, in effect, to little more than an introduction to twentieth-century historiography, belying Professor Vardy's stated aim. Similarly, he does not deal with the post-1945 Marxist period of Hungarian history-writing because it is "too close in time for an objective evaluation" (p. 217)—a notion that is open to question.

With such fastidious cavils out of the way, it must be granted that the substance of the book is a pioneering effort that fills a real gap. It is an extremely useful guide and a rich source of information for students of Hungarian history who do not command the language. There is much of value, too, for those who read the language and are already familiar with the basic Hungarian works on the subject, especially those of Domokos Kosáry, Bálint Hóman, Gyula Kornis, and István Dékány, among others.

The threefold focus of the author's attention is the positivist school of late nineteenth- and early twentieth-century writing, the *Geistesgeschichte* school of Gyula Szekfű, and Elemér Mályusz's ethnohistorical approach. All three are precisely defined and given nicely balanced and rather sophisticated analyses. Vardy offers his readers a

careful exposition of the origins of Hungarian scientific historiography and at the same time deals in an informative, interesting, and comprehensive manner with several lesser twentieth-century trends.

The book makes a considerable contribution to the understanding of Hungarian historical science and is an altogether commendable and welcome work.

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MARTIN KVETKO and MIROSLAV JÁN LIČKO, editors. *Žbôrník úvah a osobných spomienok o Slovenskom národnom povstaní* [Anthology of Personal Recollections of the Slovak National Uprising]. Toronto: Stála Konferencia Slovenských Demokratických Exulantov. 1976. Pp. 434. \$10.80.

What is officially called "The Slovak National Uprising" took place in the early fall of 1944. It was staged against the rule of the Hlinka Party in Slovakia and its Nazi protectors by a coalition of non-Communist and Communist civilians and military. The Uprising has been hailed as one of the most important events in modern Slovak history. Its military contribution to the Allies' victory over the Third Reich was slight. The Allies, Western and Eastern, were taken by surprise; and as the Slovak action was embarrassing to their cooperation—and furthermore, endangered the Soviet designs for Central Europe—neither side was overjoyed by the event.

The main importance of the Uprising lies in its assertion of the existence of an independent Slovak nation which jumped off the Nazi bandwagon. Historico-political writing about the significance of the undertaking started almost simultaneously with the first shot. Since then it has grown into an avalanche, and the number of publications exceeds greatly the number of German soldiers killed during the Uprising.

Czechoslovak historians expropriated the Uprising entirely for the Communists—with all the vicissitudes of the ever-changing party line. The *Žbôrník* is a feeble attempt by Slovak anti-Fascist exiles in the West to combat this Communist usurpation. Feeble, because most of the authors had neither the sources nor the training for the task; the majority of contributors put down their own reminiscences, some in an eloquent publicistic language and style, others in a rather pedestrian manner. Several of the articles have already seen print elsewhere. Two scholars stand out: Miroslav J. Ličko and Ladislav Lipscher. Ličko has three major essays: one putting the Uprising into a perspective, the second discussing the Allies' assis-

tance, and the last recording the actions of a Partisan brigade. It is a pity that he has not consulted British and American documents for his second article; materials I have seen would necessitate a further reinterpretation of Western "assistance." The East is dealt with properly. Lipscher sheds light on the Jewish participation in Slovak resistance—a topic frequently besmirched by the anti-Semitic press in postwar Slovakia. Jews fought in Slovak resistance in numbers higher than their proportion in the population. Lipscher should have emphasized more strongly the particular Jewish moment, so evident in testimony of veterans collected in Israel. Among the non-scholarly contributions one should mention the unfinished chapters from the *Nachlass* of the late Jozef Lettrich, the Chairman of the Slovak National Council, and that of Michal Sečanský, a commander of a Partisan brigade who offers a few insights into the way the Soviets and the Communists managed guerrilla activities in Slovakia.

The editors of the *Žbôrník* have compiled an uneven book. Its strength is in presenting a point of view different from that of the Communist historians in the East, and from that of the Hlinka Party exiles in the West.

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M. K. DZIEWANOWSKI. *The Communist Party of Poland: An Outline of History*. (Russian Research Center Studies, number 32.) 2d ed.; Cambridge, Mass.; Harvard University Press. 1976. Pp. xii, 419. \$17.50.

The first edition of M. K. Dziewanowski's book was a pioneering work. It was the first systematic study (in either English or Polish) of the history of Communism in Poland. It reviewed the developments from the very inception of the Marxist movement there in the late stages of the nineteenth century, through the upheaval against the Communist system in 1956, and to the partial liberalization carried out by Władysław Gomułka. The volume was carefully researched, well organized, and balanced in its judgments. As such, it was well received by the reviewers and was widely adopted as a basic text in university courses dealing with Polish history and politics.

The second edition is supplemented by two additional chapters covering the retreat from Gomułka's reforms, the upheaval against the Communist system in 1970, and the early years of the rule of Edward Gierek. Like the older part, the new material is informative, lucid, and objective. Yet the second edition is disappointing. Although seventeen years have passed since the appearance of the original volume it was not revised; the first fifteen chapters were simply reproduced from the

edition. No attempt was made to include the new information on the history of Polish Communism, despite the fact that in the 1960s much revealing material on the subject came to light. Even the bibliography was not updated.

One can surmise the reasons for the appearance of a second edition in this form; reproducing the old part intact and merely adding two chapters on the latest developments was certainly the cheapest method of keeping a popular textbook "current." This approach, particularly on the part of such a reputable publisher as the Harvard University Press, is nonetheless to be regretted. The students receive a textbook which appears to be up-to-date, but which in fact is largely obsolete.

ADAM BROMKE
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PETER RAINA. *Stosunki polsko-niemieckie, 1937-1939: Prawdziwy charakter polityki zagranicznej Józefa Becka* [Polish-German Relations, 1937-1939: The True Character of Józef Beck's Foreign Policy]. London: Oficyna Poetów i Malarzy. 1975. Pp. 172. \$8.00.

First a few words about the author. Born in Kashmir and educated in the USA at Clark University, he also studied in Geneva, at Oxford and Cambridge, received his Ph.D. in Warsaw, and has published works in German and Polish. At present he is a Research Fellow at the Osteuropa Institut of the Free University, Berlin.

This book deals with the activity of Polish Foreign Minister Józef Beck and is dedicated to the memory of the "great Polish patriot." But the work is not completely a eulogy of Beck: Peter Raina very strongly disagrees with him on the final solution of the Cieszyn problem in September 1938. In general, however, he presents Beck as the only statesman of that time who definitely opposed Hitler. Regarding Beck's speech in the Polish Sejm of May 5, 1939 Raina writes: "Till that time the Chancellor [Hitler] never met such a strong refusal as that from the Polish Government and the press and thank God it was done. . . . Hitler understood strong language only and Beck knew how to use it. . . . In time when appeasement flattered Hitler, Beck's language was a strong language, and Beck's foreign policy was a great policy" (p. 70).

Regarding Polish-Soviet relations Raina states that the Soviet-German Pact of August 1939 together with its secret protocols not only precipitated World War II but made it inevitable in 1939; Poland became the direct victim of the politics of the great powers (p. 98). As for Beck, Raina points out that he did not conclude any open or secret pact against the Soviet Union, although Hitler

proposed such an arrangement to him more than once. Historians find this a tragic irony because it was the USSR, and not Poland, that concluded a secret pact with Nazi Germany. This is evidence that not only Hitler, but also the Soviet Government, was guilty of war crimes (pp. 106-08).

The hero of Raina's book is Beck. Except for his policy relating to the case of Cieszyn, the author rates Beck at the top of European politicians; he was the one who opposed Nazi dictatorship in 1938-39 and frustrated Hitler's intentions till the very end. Beck in 1939 honestly observed the treaties with Poland's neighbors, the Soviet Union and Germany. When the majority of foreign ministers in Europe chose at that time to remain inactive on the sidelines, Beck was fighting for the very existence of his country. His morality was definitely higher than that of his contemporaries (p. 130).

Beck really was an outstanding man among European statesmen. A study of the personality of this Polish foreign minister still awaits its historian, and this book will make a fine contribution to a future biography. It does have some shortcomings. There is no index, some expressions in Polish are not clear, and some recent publications (e.g. Szembek's Diary in four volumes) were not used. But to his credit Raina has introduced some new material, namely the interesting and hitherto unknown reports of Polish consuls in Germany in 1938-39. In general this is a very fine approach to the character of Beck which will be widely used by historians working on the origins of World War II.

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ZENOBIUSZ KOZIK. *Partie i stronnictwa polityczne w krakowskim, 1945-1947* [Political Parties in Cracow, 1945-1947]. Cracow: Wydawnictwo Literackie. 1975. Pp. 467. 60 Zł.

Zenobiusz Kozik, a specialist on the regional history of the working-class movement, has undertaken an analysis of political parties in Cracow, the province which government officials after World War II dubbed the "Polish Vendée." In the years 1945-47 the direction of Poland's evolution still seemed not entirely determined, but even at that time—Kozik acknowledges—there was no "free play" in politics, given the realities of the country. There existed instead only a margin of political freedom of which several parties took advantage: the Communist PPR (Polish Workers Party) and its allies—PPS (Polish Socialist Party), SL (Peasant Party), and SD (Democratic Party), and by the PSL (Polish Peasant Party) and SP (Labor Party) established after the formation of

the Government of National Unity in June 1945. Symbolic for its inclusion of Stanisław Mikołajczyk who had returned from exile in London, the new government was seen as a signal that the margin would be broadened. There followed a revitalization of political life, and in the Cracow province Mikołajczyk's PSL grew so dynamically that its membership exceeded by far that of all other parties combined. Many a politician envisioned the emergence of a system in which a legal organization's access to power would be the reflection of its popular support, but this was the sort of democracy that the PPR, its grip firm on all instruments of coercion, was determined to prevent from materializing.

In the increasingly violent confrontation that followed, from the fall of 1945, with the PPR and PSL as the main antagonists, the former countered the program of liberalism with that of people's democracy and called for a common slate of all parties in the approaching elections. The conflict ran its course with an overwhelming victory for the PPR and its three allies in the elections of January 1947, but the author makes it clear that the turning point had been the referendum in June of the previous year. The referendum campaign, punctuated by arrests and the ban on the PSL in some areas of the country, had precipitated the disintegration of the legal opposition. Even though in Cracow the PSL had won the referendum impressively, the effect was to bring the party there under especially severe pressure. With the PSL disappearing after the elections, the year 1947 was marked by a drive for the unification of the Marxist-Leninist PPR and Marxist PPS. It was resisted by some Cracow Socialists, but the PPR used its influence to strengthen the pro-unification elements within the sister party, thus creating an essential precondition for the new era of political stabilization.

This narrative is well documented, and among the records quoted are those from the Ministry of Internal Affairs' archives. The reader is presented with a mass of statistics on every party and its ups and downs, month by month, county by county. These data also provide some insight into the working of the power structure in these years. The PSL, while dominant on the village or township level, was little represented in county government and only symbolically at the provincial level. Likewise, Kozik is quite informative about the extent of the opposition to the PPR in the ranks of its allies. He tells less about how this opposition was overcome and tends to leave the answers to the reader's guesswork. In the case of the PPS, he indicates that crucial decisions for its Cracow branch were taken by the central party authority, but this does not explain why one of the

Socialist leaders, Edward Osobka-Morawski, on a visit from Warsaw also came out against the premature unification.

The presence of such gaps does not fit well with Kozik's stated assumption that the period is a closed chapter. But then he also reminds the reader that not all sources have been made accessible.

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FRANCISZEK KAMIŃSKI. *Religione e chiesa in Polonia, 1945-1975: Saggio storico-instituzionale*. Padua: CESEO. 1976. Pp. 157. L. 6,000.

The real purpose of this work, not clearly expressed in the title, is to "study the relations between Church and State in Communist Poland . . . by placing these relations within the context of the social, political, and historical development of religious questions within the nations of the Socialist camp" (p. 7). Such a study is clearly needed given the paucity of works on this topic in any Western language. Unfortunately, Franciszek Kamiński fails to provide the historical analysis which he promises and which this subject deserves.

Religione e chiesa in Polonia begins promisingly enough with a discussion of the concept of religious freedom in the Marxist-Leninist tradition, a tradition which declared that it could not treat such freedom as absolute and intended to "eliminate gradually the religious view of the world from the consciousness of society" (p. 8). The Communist government of Poland after World War II embraced that vision and thus came into conflict with the Roman Catholic Church of Poland, an entrenched institution which enjoyed enormous prestige among the common people and which had grown accustomed to a privileged status under the former regime.

The new government attempted to undermine its formidable rival by imposing upon it the Communist conception of "separation of Church and state." In practice, this meant abolishing religious instruction in the schools, confiscating Church properties, trying to force the Church out of all political activities, and closely regulating seminaries and the construction of new religious edifices. The regime declared in its constitution of 1952, and reaffirmed in the amended version of 1976, that religious freedom was subordinate to the interests of the "working class" of which the party was the sole custodian. Since the upheavals of 1956 in Poland and the accession of Pope John XXIII in Rome the climate has changed, and a genuine *modus vivendi* now seems possible. The author concludes that a spirit of "friendly cooperation"

must replace the former policy of "distance" between Church and state in Poland if that nation is to achieve true social integration and economic modernization.

Unfortunately, Kamiński fails to support his conclusions with an adequate analysis of the issues he himself raises. Although he has a formidable grasp of the sources dealing with the legal and constitutional questions, he does not investigate the actual power structure of either the Church or the party, nor does he explore the inner life of either ideological community. His work is limited almost entirely to formal constitutional structures, public pronouncements, and changes in legislation. Although he attempts to analyze how the law works out in actual practice, he fails to relate legal issues to social, political, and religious realities, without which there can be no understanding of the subtleties of church-state relations in any nation of our century.

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JOHANN DIETRICH VON PEZOLD. *Reval, 1670–1687: Rat, Gilden und schwedische Stadtherrschaft*. (Quellen und Darstellungen zur Hansischen Geschichte, New Series, number 21.) Cologne: Böhlau Verlag, 1975. Pp. 391.

The once influential Baltic German school of historiography is still alive today although its character has considerably changed. Long gone are some of its most notable representatives, such as Carl Schirren, Theodor Schiemann, Leonid Arbusow, Sr., and Leonid Arbusow, Jr. More recently it has suffered the loss of Reinhard Wittram and Hans Rothfels. Despite the fact that the school has lost much of its influence since the proclamation of independence of the Baltic countries in 1918 and even lost its home ground in 1939, it is still continuing its activities on German soil, supported by the West German government and universities. It has changed its character considerably and has found a common basis of cooperation with non-German Baltic historians.

Unfortunately, the Baltic German school of historiography has also diminished in numbers and output. Still some of its losses have been recouped by new "converts" to Baltic history among native German historians and a new generation of historians whose parents were Baltic Germans but who have grown up and studied in Germany. The new "converts" seem to be generally more broad-minded and democratically spirited than most of the members of the previous generation of Baltic

German historians. On the other hand, they have lost nothing of the industrious and painstaking research method for which their predecessors gained richly deserved fame. Those predecessors, however, greatly surpassed them in one area—a thorough mastery of native Estonian and Latvian languages. If the new generation wants to be better than the previous one in all aspects, it must learn local Baltic languages.

Johann Dietrich von Pezold belongs to the youngest generation of the Baltic German school of historians and is somewhat handicapped by not being personally familiar with the Baltic area, but he has overcome this handicap by his great diligence and an instinctive feeling for Estonia which he has developed in the Baltic German environment on West German soil. He has made his debut with a research study of highly complex problems. Von Pezold traces the development of intricate relationships between the City Council, the Great Guild representing merchants, and the Little Guild representing artisans and craftsmen—all of these institutions dominated by Germans—in the great old Estonian trading center and port of Reval during the Swedish period when King Charles XI was introducing and strengthening his absolute rule.

The majority of the population outside Reval consisted of Estonians under German rule since the Middle Ages, but in more recent times the Germans themselves had been subjugated by Swedes. Faced by a new dilemma, the Germans were trying their best to perpetuate their leading position in Estonia (and Latvia) while trying to adjust their institutions to changing times. The struggle ended in 1687 when Charles XI appointed a Royal Burgomaster, thus ending not only various sources of conflict between different German interest groups in Reval but also the autonomy which the German city fathers had previously enjoyed.

Von Pezold has produced a commendable work, utilizing not only a wealth of published sources but also a great deal of unpublished material he found in Swedish archives as well as in the archives of Reval, which have found their way to Germany. The author is thoroughly familiar with his subject matter; his book reads well, but his extensive footnotes at times overwhelm the reader—often covering as much as one half the page and practically pushing the text into the background. The book will be of interest not only to students of Baltic, German, and Scandinavian history, but also to those interested in the development of European cities in early modern times.

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E. F. GREKULOV, editor. *Religiia i tserkov' v istorii Rossii* (*Sovetskie istoriki o pravoslavnoi tserkvi v Rossii*) [Religion and the Church in Russian History: Soviet Historians on the Orthodox Church in Russia]. Moscow: Izdatel'stvo "Mysl'." 1975. Pp. 255. 1 r. 08 k.

This book is a compilation of sections of already published books and articles by Soviet historians dealing with Russian Church history from the time of Russia's Christianization (989) to the Revolution of 1917. Since it was published by the Institute of "Scientific Atheism" of the Soviet Academy of Social Sciences, it is obviously ideologically biased. Still, this anthology deserves attention as it is one of very few books published in the USSR in recent decades which provides an over-all survey of the more than nine centuries of the Russian Church. Its fifteen "contributors" (of whom all but one are deceased) can be divided into three distinct groups. First, the academic historians of reputed scholarship—Bakhrushin, Grekov, Smirnov, Tikhomirov, Veselovskii, and, to a lesser degree of scholarship, Alefirenko and Budovnits—whose works, although conditioned by the Soviet ideological approach to history and, especially, to Church history, are of high academic standard. It may be added, however, that their "contributions" to this book have been abridged and occasionally censored. The second group consists of prolific historical writers but all faithful Marxists specializing in "exposing" pre-revolutionary "abuses" by the State and the Church—Bonch-Bruевич, Lenin's friend and later director of the Museum of Atheism; Gernet, author of endless works on tsarist prisons; Dmitriev, who still exposes the Russian eighteenth-century Church; and Pokrovsky, Russia's first Marxist historian and Soviet Deputy Commissar of Education for fifteen years. Finally, the last set of "contributors" is made up not even of historians but of professional antireligious activists—Ivanov, Putintsev, and Platonov, the latter formerly bishop of the so-called "Living Church," a creation of the Soviet anti-church apparatus; Platonov later revealed himself to be a militant atheist.

This volume consists of six parts: the Christianization of Russia; the Church in feudal times (the fourteenth and fifteenth centuries); the Muscovite sixteenth and seventeenth centuries; the eighteenth century; and, finally, "The Church in the Service of Absolutism in the Nineteenth and Early Twentieth Centuries." Characteristically enough, the earlier periods are treated in chapters taken from the works of distinguished scholars, while the eighteenth, nineteenth and early twentieth centuries are handled by the professional atheistic activists and are of little scholarly value.

Despite its obvious bias in conception and editing, E. F. Grekulov's anthology has certain merits, especially those sections dealing with the medieval period. Bakhrushin and Grekov provide very useful accounts, albeit abridged by the editor, on the Christianization of Russia, the spread of Christianity and the new Christian culture in the eleventh and twelfth centuries. Quite valuable are Tikhomirov's chapters on letters and the arts in early Russia, on Russian secular and regular clergy, and on Russian cultural life in the fourteenth to sixteenth centuries, as well as Veselovskii's "Transfer of the Metropolitan See of the Russian Church from Kiev to Moscow," in which he takes issue with Kliuchevskii's view that Metropolitan Archbishop Peter's coming to Moscow was not accidental but conditioned by Moscow's growing eminence.

Although all items of the volume are merely reissues of earlier publications, several, especially those dealing with the eleventh to sixteenth centuries, provide a systematic presentation of the role of the Church in the Russian past. Moreover, these reprints are of intrinsic value because the books or periodicals in which they originally appeared have been out of print for several decades and thus not readily available to today's readers and scholars.

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L. G. BESKROVNYI *et al.*, editors. *Drevnerusskie kniazhestva X-XIII vv.* [Early Russian Principalities in the Tenth through the Thirteenth Centuries]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 301. 1 r. 52 k.

In 1951, A. N. Nasonov published a very important work ("Russkaia zemlia" i obrazovanie territorii drevnerusskogo gosudarstva) which investigated geographic aspects of the development of the Kievan Russian state. By meticulously studying references to ninth through thirteenth century settlements he traced the evolution of the "Russian Land" of the middle Dnieper valley into the far-ranging Kievan Russian state. Nasonov's work was important not only because of his careful scholarship on the incorporation of areas into the Kievan state, often settlement by settlement, which is still basic, but also because of the thesis to which the sources led him, namely, that the development of the early Russian state was a result, not of the growth of a unified market system in Russia, or of deliberate colonization, but rather of the spread of governmental authority into regions where clan and tribal authority reigned. The early Russian state was formed, basically, he felt, by planting permanent governmental-administrative centers (*hogosti*) in

outlying areas and thus bringing the local inhabitants under the central government by bureaucratizing "tribute and justice," i.e., tax collecting and legal power.

Without clearly enunciating such a purpose, the book under review attempts to bring Nasonov's study up to date by amending some of his factual material, often on the basis of recent archeological work on early Russian towns, and, indeed, by questioning some of his specific interpretations. This book, which is a collection of essays by various authors on individual "lands" as they develop into principalities (Kiev, Chernigov, Pereiaslav', Novgorod, Polotsk, and Smolensk are covered in this volume; other areas will be treated in a subsequent one), is, indeed, for the most part merely a supplement to Nasonov's investigation. Unlike its model, however, it maintains no unified thesis or balance in depth of coverage. Some of the contributions are but simple summaries of recent monographs or lists of addenda, while others treat a specific subject in some depth. Kuza's article on the Novgorod Land, for example, suggests some interesting new ideas on Novgorod as the seat of a tribal confederation active in the "calling of the Varangians" to rule and also on the nature of the relationship between the boroughs of Novgorod and the territories. In turn, the final article in the collection, by Pletneva, deals not with a Russian principality but rather, in terms of historical geography, with the simultaneous state-building among the nomadic Cumans of the south Russian steppe and goes considerably beyond the previously standard work of K. V. Kudriashov, *Polovetskaia step'* (1948).

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JOSEPH L. WIECZYNSKI. *The Russian Frontier: The Impact of Borderlands upon the Course of Early Russian History*. Charlottesville: University Press of Virginia. 1976. Pp. x, 108. \$9.50.

In addition to his well-known translating and editorial activities, Joseph L. Wieczynski of Virginia Polytechnic Institute and State University has now expanded two of his numerous articles on early Russian history (*Russian Review*, 1972 and 1974) into this slim monograph. Its object is to apply the Turner thesis about the relationship of the American frontier to the development of American society and political institutions to Russian history through the end of the seventeenth century.

Wieczynski claims that Kievan Russia, and Novgorod during the Mongol period, were frontier societies, characterized by high social mobility and political democracy. The closing off of the frontier to the south, especially by the Mongols, resulted in

drastic social and political change; by the end of the sixteenth century serfdom and autocracy dominated Muscovy. The opening of new safety valves in the sixteenth century, the Volga steppe in the south and Siberia in the east, could no longer influence the development of the Russian center. Nevertheless, the Cossacks and Siberian society retained elements of the frontier ethic of Kiev long thereafter. Wieczynski's conclusion suggests, among other things, that to her detriment Russia never achieved a compromise between the "orderly but repressive State" and the "free but uncensored frontiersman"; this failure mitigated the positive benefits which Turner ascribed to the frontier in American history.

Wieczynski acknowledges in his preface that evidence to validate his assertions is often meager, that the monograph is based almost exclusively upon secondary works rather than his own original research, and that specialists will no doubt take issue with many of his generalizations. Still, he expresses the hope that the ensuing discussion will contribute to a better appreciation of the role of the frontier in early Russian history.

Wieczynski has read widely and thought deeply; his book is forcefully argued and vividly written. His description of life in Siberia could easily rival any dime-novel view of the Wild West, and his suggestion of the link between the frontier and the type of society found in Viatka and other peripheral regions is intriguing. On the whole, however, I found most of Wieczynski's social analysis both unwarranted by the evidence and unconvincing; he often attributes to the influence of the frontier events or processes better explained otherwise; and several of his analogies to modern Russian history seem to distort more than they illuminate. Wieczynski's synthesis of existing scholarship has already been superseded on a number of points, and one does not need to adhere to a Jeffersonian faith in yeomen farmers to be skeptical of his explicit inverse relationship between agriculture and democracy. Yet in the last analysis the book does succeed in stimulating the thinking of the reader about the role of the frontier in early Russian history, and this, after all, is the author's goal.

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V. A. ALEKSANDROV. *Sel'skaia obshchina v Rossii (XVII-nachalo XIX v.)* [The Village Commune in Russia from the Seventeenth to the Beginning of the Nineteenth Century]. (Akademiia Nauk SSSR, Institut Etnografii.) Moscow: Izdatel'stvo "Nauka." 1976. Pp. 321. 1 r. 61 k.

V. A. Aleksandrov's book on the peasant commune in manorial villages has received lavish praise in the Soviet Union. It is, indeed, a work of great merit, but less than a comprehensive synthesis. It consists of six inter-related essays: a historiographical survey; analyses of the relationship between communes and manorial administrations and of the composition and procedures of communal assemblies; extended discussions of the allocation of land and obligations and of recruitment into the army; and a brief treatment of the communes' maintenance of the authority of parents and husbands. Aleksandrov has deliberately excluded discussion of the commune as an agency of protest and revolt, and thereby sloughed off questions of social psychology, such as the peasants' conception of the commune, that are important for the themes he chooses to treat. Nor does he mention the commune as a bearer of agricultural traditions and determinant of the choice of crops, timing of sowing and harvesting, and the like.

Furthermore, the five substantive chapters rest almost entirely on two kinds of source: some fifty sets of regulations composed by squires and about five hundred resolutions (*mirskie prigovory*) passed by communes. Except for noting that the sources he used pertain overwhelmingly to peasants rendering dues in cash, rather than in labor, Aleksandrov does not seem to have inquired whether the sources he used are representative of manorial villages. Yet the communal assembly in which all heads of household participated makes no appearance on these pages; all the communes Aleksandrov has studied convened assemblies of delegates from a number of villages and often delegated broad authority to standing or *ad hoc* commissions.

Aleksandrov shows that many communes had a considerable, although diminishing, measure of autonomy. He even refers (like Genovese with respect to American slaves) to the "rights" enjoyed by communities of bondsmen. Yet he is very far from idealizing the commune. Under serfdom communes enjoyed some autonomy and some bargaining power because they effectively imposed discipline on their members and upheld the squire's economic interests as well. Where the interests of the squire and the peasant community as a whole coincided, the commune was merciless. It sought to ensure that every household could bear its share of the village's obligations to the squire and to the state. Hence it reallocated land, but also liquidated economically weak households. The male members of these households would be sent off to the army, so that the village could meet its draft quota without disrupting the balance of persons, resources, and obligations in the strong households. The wives of these recruits, like orphans and other economically dependent individ-

uals, could not expect much help from the commune.

Sel'skaia obshchina is especially welcome because the subject, once a staple of political and historiographical controversy, has been neglected for decades. It offers valuable insight into such matters as the actual burden of taxes on peasants and the dissemination of bureaucratic modes among both nobles and peasants. It is a work of rigorous, industrious, and sometimes inventive scholarship, which every historian of Imperial Russia should read. Yet its limitations, acknowledged and unacknowledged, present an incentive for further research.

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BERND KNABE. *Die Struktur der russischen Posadgemeinden und der Katalog der Beschwerden und Forderungen der Kaufmannschaft (1762-1767)*. (Forschungen zur osteuropäischen Geschichte, volume 22.) Berlin: Osteuropa-Institut. 1975. Pp. 396.

In the style of V. M. Kabuzan's monumental work on population statistics, this is a quantitative study of town communities in Great Russia and Siberia in the early 1760s. It shifts from a purely descriptive account, region by region, of administrative development to a parallel exploration of "*sein und Bewusstsein*" of *Posadgemeinden*. The result is highly condensed. There is no single theme, but rather a sweeping investigation of topics ranging from occupational patterns and political attitudes of town dwellers to determinations of town size and structure. This book is a major contribution, with valuable and accessible tables.

Utilizing a wide variety of sources, Bernd Knabe refutes many previous concepts. He finds, for example, that illegal trade was so widespread that "it is not permissible to speak of the dominance of the merchantry on the internal market" (p. 291). Competition between social groups for control of local trade, he argues, actually stood in the way of the evolution of cooperative commercial relations between town and country. Illegal trade and an almost universal lack of capital funds among the merchantry were prominent among the reasons for the nominal value of commerce in most Russian towns in the middle of the eighteenth century.

The merchants' response to illegal trade and to the maze of legal restrictions on their freedom is the subject of the final section of Knabe's work. In disagreement with recent scholarship, he concludes that merchants had a sense of community, or class, and in their grievances expressed "*bürgerlich-kapitalische*" interests. His evidence is exhaustive. He relies extensively, although by no means exclusively, on the instructions to Catherine II's

Legislative Commission of 1767–68 and cites archival sources which have never before been used.

Knabe's book is the most ambitious study of Russian *posad* settlements since Kizevetter's work at the turn of the century, but it is not definitive. Its weaknesses lie in its narrow chronological framework and its strictly quantitative format. One misses the imaginative and textured study of social life that is now associated with the *Sixième Section*. Its shortcomings aside, this is a pioneering effort which will challenge assumptions and encourage further research.

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L. G. BESKROVNYI. *Russkaia armia i flot v XIX veke: Voenno-ekonomicheskii potentsial Rossii* [The Russian Army and Navy in the Nineteenth Century: Russia's Military-Economic Potential]. Moscow: Izdatel'stvo "Nauka." 1973. Pp. 616. 2 r 79 k.

L. G. Beskrovnyi is eminently qualified to write this history of the Russian military during the nineteenth century. Like his earlier works on the tsarist army, this book is based upon an impressive collection of archival sources. In this volume of essays, the author examines the historical evolution of such important aspects of the military as organization and administration, recruitment and training, production and procurement of weapons and supplies, transportation and communication, military appropriations, and sociopolitical movements within the army and navy. The book also includes a lengthy essay on the navy which does not seem to integrate with the rest of the manuscript.

As the subtitle suggests, the author has attempted "to demonstrate on what sort of economic basis the development of the country's armed forces took place, how the military-economic potential changed during the transition from a feudal serf-based order to a bourgeois one, and how it [the transition] affected the development of the military and naval arts. And, on the other hand, how the army and navy had an effect upon the country's economic development" (p. 5). The author's treatment of each topic depicts the growing crisis of the serf-based army during the first half of the century. Serfdom and the social order based upon it made impossible the rapid expansion of the army in time of war, the integration of technology or meaningful military reforms. Even after the Crimean War had demonstrated the bankruptcy of the old military system, reforms in many areas were slow in coming.

Beskrovnyi clearly leaves the reader with the impression that the process of military modern-

ization in the second half of the century was incomplete and unsuccessful. In matters of army organization, personnel, and training he indicts tsarist statesmen for protecting the gentry's monopoly within the officer corps at the expense of army professionalization. For Beskrovnyi, the reforms of D. A. Miliutin did not give birth to a modern mass army because of the many compromises that the tsars and conservative figures imposed upon Miliutin's plans.

The author's most interesting points concern the relationship between national economic development and military power. Military requirements contributed to the growth of Russian heavy industry in the second half of the century. In spite of overwhelming economic backwardness throughout the economy, some sectors maintained a qualitative level of development equal to that of the most advanced industrial countries, for example, the small arms industry, cannon works, and powder factories. Quantitatively, however, Russia fell further and further behind these same powers with regard to the potential for the expansion of the arms industry in case of a general European war.

Given the scope and complexity of the topic and the author's determination to treat each theme's historical development over the entire century, it should not be surprising that the reader is left at times with the impression that certain themes could have been more fully discussed and better integrated with other topics. These comments aside, *Russkaia armia i flot v XIX veke* will be welcomed by all students of military history.

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V. A. GEORGIEV. *Vneshniaia politika Rossii na Blizhnem Vostoke v kontse 30—nachale 40-kh godov XIX v.* [Russian Foreign Policy in the Near East from the End of the 1830s through the Beginning of the 1840s]. Moscow: Izdatel'stvo "Moskovskogo Universiteta." 1975. Pp. 200. 90 k.

With rare exceptions archival access by American scholars in the USSR has been limited to those few participants on official academic exchange programs. Even so, the Archives of the Ministry of Foreign Affairs (AVPR) have remained closed: for all intents, then, Soviet publications which utilize AVPR documents provide the only appreciation of treasures locked from sight. And few such works have appeared in recent years.

Thus the publication of a monograph by an instructor on Moscow State University's History Faculty must be welcomed, the more so because the Eastern Question it addresses has long been of considerable interest to Western scholars. The work contains an introduction, a lengthy survey of

sources and of historical literature, four main chapters, a brief conclusion, and seven pages of bibliography. More works are listed in English and in French than in Russian (there are none in German), but almost nothing is noted after 1968; and here, as in the text, there is an annoying inability to reproduce foreign titles accurately. The main primary sources used by Georgiev include the reports of Russian ambassadors in Constantinople, London, Paris, and Vienna (for the years 1838–41); Minister of Foreign Affairs Count Nesselrode's instructions to his diplomatic representatives and yearly reviews to Nicholas I (1839–41); and Nesselrode's correspondence with foreign ambassadors in St. Petersburg (apparently 1841 only), as well as relevant published materials of Western European parliamentary and diplomatic leaders and statesmen.

Since Russian diplomatic materials are largely unpublished, Georgiev's work should be a goldmine. He telegraphed his study in an historiographical essay in *Voprosy istorii* in 1970 in which he declared that no works had been written on the history of the Second Turkish-Egyptian conflict, 1839–41. The monograph is intended to fill this gap; and, in fact, Georgiev periodizes Russian foreign policy by these crises in the Near East, and so narrowly that he ignores other important and related international questions faced by Russia (and by the other European states) during this period. This is but one of the serious weaknesses of the work.

Georgiev states that he intends to study the reasons for the "new interference" of the European powers in the Near East beginning in 1839 (ignoring the thesis developed forty years ago by Philip Mosely that the trend began the previous year); he quickly attributes it to economic competition (primarily English-Russian), but fails to develop this unremarkable thesis. Instead, Georgiev devotes most of the work to the course of diplomatic activity (again, primarily English-Russian maneuverings), and concludes that Russia fundamentally changed its course of foreign policy in 1839, and thereafter lost at every diplomatic turn. The great change lay in Russia's surrender of the advantages (mostly imagined) of the 1833 Treaty of Unkiar-Skelessi in favor of a collective European guarantee for the Ottoman Empire. Georgiev does cite Nesselrode's claim that the London Conventions represented successes for Russian foreign policy (p. 187), but follows immediately with a sentence that is reminiscent of the Russia-firsters Nesselrode struggled against at the time. It is regrettable that, with all the documents at hand, Georgiev makes no attempt to explain the change of policy, either in terms of court politics or as part of Nesselrode's long-range goals.

Although the monograph is packed with references to AVPR documents, the author often used his material only to certify facts verifiable elsewhere (e.g., information about dates and places of battles). Moreover, in spite of numerous cautionary statements in the introduction about the dangers involved in the use of various diplomatic and other sources, Georgiev almost never applies critical analysis to the material he quotes. And there are gross mistranslations of English-language materials. Georgiev claims that Mosely's work was written "wholly on the reports of Ponsonby, the English ambassador in London [sic]" (pp. 48–9). There are no references to Ponsonby (whom Georgiev later correctly places in Constantinople) on the page cited by Georgiev in his footnote. But Mosely himself cited many AVPR documents (a number of which Georgiev also uses), and thanked Soviet archivists for their "everwilling cooperation" "by admitting me [Mosely] to work freely in the diplomatic materials of the period 1838–1842." Leaving aside the obvious embarrassment related to any acknowledgment that an American got there first, such (and many other) crude falsifications raise doubts about the technical training of Soviet scholars. Judging by Georgiev's inaccuracies in using documents which can be cross-checked, then, one must conclude that Western scholars must reserve judgment on the reliability of the AVPR citations and, thus, about the interpretation. In an historiographical article which appeared in *Voprosy istorii* in 1968 Georgiev pleaded for the publication of more archival documents; that request is seconded. Georgiev claims that one of his goals is to correct the errors and tendentiousness (his word) of Westerners' works; it is regrettable that this reviewer must do a similar job for him. As it stands the work contributes little to our knowledge of the Eastern Question or to our acceptance of Soviet historical scholarship.

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RICHARD S. WORTMAN. *The Development of a Russian Legal Consciousness*. Chicago: University of Chicago Press. 1976. Pp. xi, 345. \$20.00.

Richard S. Wortman's new book deals with the important and elusive subject of the growth of legal consciousness in Russia in the decades preceding the fundamental judicial reform of 1864. More precisely, it "is an inquiry into the changes in composition and attitudes that took place in the Russian judicial administration" (p. 5). Concern with the composition leads in particular to numer-

ous tables meticulously summarizing information on the Russian prereform legal bureaucracy, especially on its social origins, education, wealth, and course of service. Preoccupation with the attitudes accounts for stimulating excursions into social psychology and intellectual history as well as for the device of carrying on the narrative largely by means of interesting, at times fascinating, although impressionistic and subjective sketches of the Russian ministers of justice and other prominent judicial officials.

Following a general introduction, the book is divided into three parts. The first, "autocracy and the law," consists of three chapters devoted respectively to "absolutism and justice in eighteenth-century Russia," "bureaucratization, specialization, and education," and "the composition of the Russian legal administration in the first half of the nineteenth century." The second part, "the men," has an introduction on "the noble legal official" and four chapters: "Russia's first minister of justice" (Derzhavin, no less); "the quiet shelter" (Ivan Dmitriev, but also others, notably Karamzin and Zhukovskii); "Count Dmitrii Nikolaevich Bludov;" "Count Victor Nikitich Panin;" and "the emergence of a legal ethos." That last chapter, perhaps the best in the book, contains a sparkling and penetrating account of the new School of Jurisprudence and its seminal role in Russian legal history. The third part, "reform," is made up of an introduction on "the old judiciary," and of two chapters, "the aspiration to legality," and "epilogue and conclusion," which features the author's *aperçu* of the judicial reform itself and of its fate. The extensive notes are unfortunately relegated to the back of the volume. The attractive book contains also a bibliography and an index. Some twenty-five or thirty misprints, mostly minor mistakes in Russian titles and transliteration, do not seriously interfere with the reading.

This is an interesting, rich, well-written, and persuasive study. In fact, the one serious criticism of it might be that it is too persuasive. It takes an effort after reading the fluent discussion of Zhukovskii, for example, to realize not only that Zhukovskii did not belong to the Russian judicial administration but also and more importantly that his general role and influence were vastly more complicated than brought out by Wortman. Again, the remarkable image of Count Victor Panin, obsessed, compulsive, cursed by the somber past of his family, seems quite compelling until one asks whether family past is what obsessions and compulsions are made of. Queries of that nature could be multiplied. But, instead of belaboring the point that some parts of the study are more convincing than others, it is appropriate to commend

the contribution of the author to a difficult and in certain ways novel subject. We are in his debt.

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ROBERT W. TOLF. *The Russian Rockefellers: The Saga of the Nobel Family and the Russian Oil Industry*. (Hoover Institution Publication, number 158.) Stanford, Calif.: Hoover Institution Press. 1976. Pp. xv, 269 \$14.95.

Robert Tolf has narrated the history of the Nobel family in Russia, from December 1838, when Immanuel Nobel, father of Robert, Ludwig, and Alfred, arrived in St. Petersburg to demonstrate his underwater mines and rubber products to the Russian military, to 1918 with the hasty exit of Immanuel's grandson, Emanuel. The inventive and administrative genius of the Nobel family has become almost solely associated with Alfred, who grew up in Russia but gained fame for his Paris-based dynamite monopoly and endowment of the Nobel Foundation, obscuring his equally talented brothers and nephew. The eldest, Robert, recognized the vast potential of the Baku oil fields, while Ludwig, with his engineering background, manufactured the Nobel wheel, the Michelin of its day, and then created the Russian petroleum industry. Ludwig developed the most advanced oil transportation and distribution network at that time by building the first oil tankers and his own fleet of railroad tankcars, constructing pipelines to carry crude oil from well to refinery. By 1900, Russian oil fields supplied forty-five percent of the world's crude oil, with the Nobel wells pumping nine percent. After Ludwig's death, his son, Emanuel, effectively steered the Nobel empire through the final turbulent years of the tsarist regime, when not only revolution, war, and strikes, but the aggressive competition of Standard Oil and Shell, threatened the family's fortune.

This book is a valuable contribution to the history of the oil industry and adds to the growing literature on foreign capital and technology in tsarist Russia. It will remain the basic work on the *Nobelevski*, what Tolf labels the Nobel period of the world's oil history, based on extensive research in Western archives and libraries, and the author's technical knowledge of the oil industry.

The dearth of Russian sources cited precludes a definitive account of Nobel family involvement with the tsarist government, Russian capitalists, and the active Baku mass labor movement. Recent Soviet publications on foreign monopoly and capital in the tsarist oil industry are not cited. Close Nobel ties with tsarist officials are frequently men-

tioned but rarely developed. Careful editing would have eliminated such errors as "Grand Duchess Vladimir," "Björke," and "Sayouzneft," which detract from, but do not diminish, Tolf's remarkable research.

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M. V. NECHKINA. *Vasilii Osipovich Kliuchevskii: Istoriia zhizni i tvorchestva* [Vasilii Osipovich Kliuchevskii: A History of His Life and Creative Work]. Moscow: Izdatel'stvo "Nauka." 1974. Pp. 635. 3 r. 34 k.

This is an important work for students of Russian historiography and of nineteenth-century intellectual history. M. V. Nechkina has given us what shall long remain the definitive Russian-language study of the most influential Russian historian of the late imperial period, Vasilii Osipovich Kliuchevskii.

In the words of the subtitle this is a study of Kliuchevskii's "life and creativity." Through exhaustive research, careful analysis, and generally thoughtful judgments, we are given a full picture of the personal and intellectual odyssey of the poor priest's son who became the leading interpreter of Russia's past for both Russians and foreigners. An essay on Kliuchevskii's treatment in subsequent historiography introduces the work. There are chapters on his childhood and early education, and his university years and early works, as well as a detailed analysis of his doctoral dissertation, *The Boyar Duma*, substantial treatment of his talents as a lecturer, and the development of his famous *Course of Russian History*. Each chapter interweaves analysis of intellectual development with personal circumstances and with the political and social changes which took place during the period 1841 to 1911. There are 859 footnotes, many of them highly illuminating, as well as an index of personal names. Since Kliuchevskii knew or was known by most of the important figures of the intellectual and cultural life of his day, this scholarly apparatus provides a rich resource for students of the period.

The author's analysis and evaluation of Kliuchevskii lacks the stridency of earlier Soviet studies. Kliuchevskii has always been a troubling member of the prerevolutionary pantheon to Soviet scholars. In general he has been seen as struggling toward an economic and class interpretation of Russian history without achieving the "scientific" perspective of Marxism. Nechkina's evaluation is basically the traditional one but with added depth and nuance. She plays down the philosophic idealism of Kliuchevskii and emphasizes his materialism and his movement toward the left

politically during his later years. It is at this point that one may differ with Nechkina's interpretation. While she writes that there are "fleeing" references (p. 176) in Kliuchevskii's works to Marx, her citations refer to two minor and unpublished works of Kliuchevskii. The fact remains that he was largely indifferent to Marx to the end of his life.

When citing the ironic aphorisms and *obiter dicta* which fill Kliuchevskii's diaries and private writings during the later years, Nechkina tends to emphasize those that support his "leftward movement." There is equal evidence of his scorn for all parties, factions, and philosophies. Thus, his vehement opposition to Maxim Gorkii's admission to the Academy is not even mentioned. There is insufficient analysis of the course he gave in methodology during the academic year 1884-85. While the famous essay on Lermontov, *Melancholy*, is mentioned, there is no analysis of the romantic assumptions found therein.

Nevertheless, these are minor omissions in an otherwise impressive work. Nechkina's biography stands as a thoughtful—even affectionate—study of one of the most notable talents of Russian historiography by one of the most impressive of contemporary Soviet scholars.

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OLGA CRISP. *Studies in the Russian Economy Before 1914*. (Studies in Russian and East European History.) New York: Barnes and Noble. 1976. Pp. x, 278. \$25.00.

Students of Imperial Russian economic history are certain to be familiar with the work of Olga Crisp. Most, however, are likely to have missed at least one of the articles in this collection since they have appeared over twenty years in a wide range of publications. One is previously unpublished, and several other have been revised.

The present volume can be divided into two roughly equal parts. The first consists of three interpretive essays on prerevolutionary industry and agriculture. The second half includes five articles on closely related aspects of late-nineteenth-century banking, financial policy, and French investment in Russia. The general essays, particularly "The Pattern of Industrialization in Russia, 1700-1914," will be of interest to all Russian historians and students of economic development in other countries. The second half, and this is not meant to be a criticism, will be of interest largely to specialists in the history of international finance and banking.

In the three initial essays, the author per-

ceptively examines some of the standard wisdom about Russian economic development: the role of serfdom, the size of enterprises, the "advantages" of backwardness, and so forth. The task is performed with sophistication and judgment, using a wide range of published sources, both primary and secondary. The author seems to be less interested in undermining established views, although this is accomplished in some cases, than in showing how complicated the real situation was. The point of view is largely that of an economist (although not of the "new economic history"), and the argument presented in those terms is generally impressive. References to social and political matters are perfunctory and frequently unconvincing. That the economic policies of the Russian government with respect to industrialization "... emerged in response to general financial, political, and military needs and not with a view to a macroeconomic optimum" is doubtless true, but hardly surprising.

Most readers will find the second half difficult and probably dull. One might wish that Crisp had chosen to write a summary essay on banking, finance, and foreign investment in the crucial last fifty years of the tsarist regime. Certainly she is the Western scholar most qualified to do it. As it stands, it is a service to the field to have these articles collected in one place, but they have little in common with the more provocative and stimulating essays of the first half.

In an otherwise well-produced book it is most unfortunate that the publisher has chosen to place the footnotes awkwardly at the back. A very wide range of sources is used and it is essential to follow the notes as one reads the text.

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L. M. GORIUSHKIN. *Agrarnye otnosheniia v Sibiri perioda imperializma (1900-1917 gg.)* [Agrarian Relations in Siberia in the Period of Imperialism, 1900-1917]. Novosibirsk: Izdatel'stvo "Nauka," Sibirskoe Otdelenie. 1976. Pp. 341. 1 r. 63 k.

As a genre of literature, this sophisticated, well-written and informative study of socioeconomic relationships in rural Siberia, 1900-17 is in the tradition of Lenin's *Development of Capitalism in Russia*. Its main thesis is basically the same: the forces at work in the country creating a market economy were also at work in Siberia, but at an accelerated rate, with the concomitant differentiation of the peasantry into a rural bourgeoisie and rural proletariat. The process was accelerated in Siberia, which is characterized as a colony "in an economic sense," because of the absence of a land-owning class, the abundance of land, the influx of peasant labor, and the tying of local agricultural produc-

tion to markets in European Russia and abroad by the Trans-Siberian railroad. The central feature, of course, is the creation of a large class of rural wage-earners, who experienced their place in the world in terms of their own class interests.

L. M. Goriushkin, looking beyond his material to 1917, makes an impressive argument, drawing on a wealth of sources including records of local agricultural societies and folklore, and presents great amounts of statistical material covering all aspects of rural economic life. To anyone interested in the history of Siberia in the late nineteenth and early twentieth centuries, the book is indispensable, but as an explanation for the behavior of the peasantry in 1917, it fails to convince. Mountains of evidence are presented concerning the objective changes in agricultural life, but very little showing how the peasants actually understood themselves and their place in the world. For his argument to be valid, the author must show that rural proletarians manifested a rural proletarian class consciousness and this he has not done. On balance, however, the strengths of the book far outweigh this weakness, and it has to be judged a first-rate scholarly work.

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A. M. DAVIDOVICH. *Samoderzhavie v epokhu imperializma (klassovaia sushchnost' i evoliutsiia absolutizma v Rossii)* [Autocracy in the Age of Imperialism: The Class Nature and Evolution of Absolutism in Russia]. (Akademiia Nauk SSSR, Institut Gosudarstva i Prava.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 348. 1 r. 46 k.

This book is an attempt to dissect the Russian government of the late nineteenth and early twentieth centuries prior to 1917. To this end A. M. Davidovich examines the interrelation of various socioeconomic classes with the government, the class origin and mentality of governmental officials and unofficial but influential pressure groups, and the mechanism of government before and after 1906. Unfortunately, the author has forced his material into a Marxist-Leninist mold. As a result, the book has several flaws.

It is tendentious and polemical. Davidovich wages a constant struggle, for example, against Menshevik "liquidators," contemporary Soviet historians, notably A. Ia. Avrekh, and contemporary Western historians, all of whom improperly understand the class basis of the autocracy. The book is riddled with clichés about the struggling proletariat, oppressed peasantry, Black-Hundred clergy, and *krepostniki-pomeshchiki* (loosely translated as landowners advocating serfdom or landowners of serf-owning mentality). Worst of all, in

order to coincide with Leninist dicta, Davidovich's conclusions often contradict or dismiss as inconsequential factual data he has provided.

Thus, although he shows that financiers not belonging to the hereditary nobility increasingly were receiving high governmental posts, he stresses that nearly all political power was in the hands of the *krepostniki-pomeshchiki*. Despite the fact that he notes differentiation among the peasantry, he maintains that all peasants raged against both the *pomeshchiki*, who restricted them, and the autocratic government. He supports this contention with statistics on peasant uprisings but does not discuss the causes of these uprisings. Although he discusses the development of a liberal wing among the nobility, he claims that fear of a popular uprising drove all nobles—and all liberal bourgeoisie—into a policy of supporting the autocracy as the lesser of two evils. Generalizations and simplifications abound: Stolypin was a reactionary; in general, ministers, the tsar, and other government figures appear to have had a homogeneous, class viewpoint; the Octobrists and Kadets supported all major governmental proposals during the Third Duma period; Russian law remained serf law until 1917. The Russo-Finnish conflict of 1908–10 is treated in a sketchy, hidebound manner.

Nonetheless, the book has redeeming features. Although archival material is sparse, Davidovich provides interesting information on the court clique, the formation of various political parties between 1901 and 1905, governmental meetings leading to the creation of the Bulygin Duma and to the institutional changes of 1906, and the composition and activity of governmental institutions prior to 1906. (This last topic, however, has been covered far more thoroughly by George Yaney in *The Systematization of Russian Government*.)

The last chapter of the book is genuinely exciting. Davidovich convincingly argues that the tsarist government was not a constitutional monarchy from 1906 to 1917, as liberals of that day and many current historians have insisted, but rather what he terms a constitutional autocracy. By this he means that although Russia, after 1906, had a constitution in the sense of an orderly form of government, the empire did not have a constitutional monarchy in the sense of there being adequate limitations on the tsar's power. According to the Fundamental Laws of 1906 the tsar shared legislative power with the Duma (and the State Council) and no longer had unilateral power to change the Fundamental Laws or control over the entire state budget. However, he retained essential executive power and significant legislative and financial power. Davidovich ignores the State Council's implementation of its function to impede legislation and there are other omissions and

distortions in this chapter. Nevertheless, the author emphasizes the confused political structure resulting from the Fundamental Laws of 1906 and the enormous power the tsar actually wielded from 1906 to 1917—points which some Western historians have noted then seemingly discarded in their discussions on the possibility of a viable Duma. Davidovich's last chapter is provocative and well worth reading.

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WALTER SABLINSKY. *The Road to Bloody Sunday: Father Gapon and the St. Petersburg Massacre of 1905*. Princeton: Princeton University Press, 1976. Pp. xi, 414. \$18.50.

On January 9, 1905, an unarmed procession of workers and their families converged from several directions on the square before the Winter Palace in St. Petersburg. Their intention was to present a petition to the tsar containing both political and economic demands. The person responsible for the plan was Father Georgii Gapon, a priest who had organized with police knowledge and approval a kind of union called the "Assembly." Groups seeking to reach the square were fired on at several points and many were killed and wounded. It has long been thought that this event, which conventionally marks the beginning of the "Revolution of 1905," produced widespread disillusionment among the Russian masses and led many to turn from peaceful to violent protest.

This incident, its background and immediate aftermath, form the subject of Walter Sablinsky's book. Like many historical monographs, it is less strong on analysis than on narrative. The treatment is in some respects conventional; the benefit of the doubt is usually given to opponents of the regime, seldom to its supporters. For example, a document prepared by Gapon is described as "extremely reactionary" (p. 88) despite the fact that it argued for the right of workers freely and openly to organize. Von Laue's conclusion that Russians were eating better (p. 6), drawn from the fact that grain and potato production increased twice as fast as the population from 1861 to 1900, is dismissed because "neither Liashchenko nor Lenin would have subscribed to this conclusion" (why not?). The conclusion that "what generations of revolutionaries" had failed to achieve "was brought home to the working masses in a few hours" by soldiers' bullets (p. 271) and that "after Bloody Sunday even the most religious monarchists among the workers tore portraits of the Tsar from the walls of their homes" (p. 274) may be true; but perhaps the possibility that the effects

were not really nationwide and were transitory even among St. Petersburg workers required examination. Since the possibility is not even mentioned, one may be reluctant to accept the conclusion as stated. It certainly is contrary to the view of the last Western scholar to devote serious attention to this episode, Sidney Harcave, in his monograph on 1905. Harcave writes that after January 9 "the great majority of the workers . . . were tired of having their problems mixed up with those of political reform; they remained monarchist and . . . were still hostile to the enemies of the regime. . . ." (Harcave, p. 96). Though Sablinsky gives a footnote to Harcave, he does not mention his conclusions.

The major question this study raises, however, is never addressed at length or in detail: that is, the question of the extent to which the spontaneous expression of grievances and demands for remedies that arose among the St. Petersburg workers coincided with revolutionary political programs. A long footnote (p. 15) challenges the "traditional" view that "left to themselves, workers become engaged in trade union activity directed primarily toward economic issues." That view was expressed by none other than Lenin himself, and it is surprising indeed not to find mention of the well-known fact. But the author identifies it with Richard Pipes and attacks it. Later, Zubatov is quoted as saying much the same thing as Lenin: "Workers are not capable of being independent; they immediately fall under alien influence." More to the point, the author proceeds to show clearly that Gapon was ill at ease with intellectuals, was out of his depth in any kind of theory, but understood the workers very well (p. 125); that members of any revolutionary party were almost nil among St. Petersburg workers (p. 174); that workers were indifferent to Social-Democratic speakers and that therefore the latter imitated Gapon in gestures and even accent in order to get a hearing at all. Then, suddenly, we are told that Gapon's movement showed "how deeply the ideas of Social Democracy and the labor movement struggle were ingrained in the workers" (p. 182), though much evidence to the contrary has just been presented. Hostility to all revolutionaries is shown in fact to have increased in the days just preceding January 9 (p. 226).

In general the religious aspect of the Gaponovshchina is slighted. The actual petition to be presented to the tsar was to a large extent in the form of a prayer, whose roots could doubtless be traced to liturgical and other ecclesiastical sources; that is not mentioned. The procession was in its form religious; it was even advertised as a *krestnyi khod*, as is noted. Gapon's weaknesses were in part his own, and in part those of many Russian clergy,

educated or miseducated in a certain way; that is not explained. Obviously, the religious habits of the workers were still strong.

The detailed narrative is instructive and, as far as I can see, impeccably done. However, we are not given much help in understanding the details of what happened. After several pages devoted to different reports of the number of casualties of January 9, the author fails to give us his own conclusion. Immediately after the tragedy, Gapon appeared at a public Free Economic Society meeting in disguise, threw it aside and spoke as a revolutionary, and vanished safely. The whole police force of the city was looking for him. How could he have escaped? The deed is not even remarked upon. When Gapon went abroad, to everyone's surprise he showed up without warning in the Geneva lair of the Mensheviks (though the SR's had just been helping him). Why? The question is not even asked.

There are a number of typographical errors, the most annoying ones in transliteration from Russian—I have counted eleven—and in the footnotes. The map needs to be related to the text; only Russian speakers will know that "Vasil'evskii Ostrov" (map) and "Basil Island" (text) are the same. But factual errors are not a problem, and in general the author has well and accurately told his story. Reviewers may still need to insist, however, that important questions be put to the historians' material, questions to which the best possible answer can come only from the scholar who knows best the sources at issue.

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G. A. ARUTIUNOV. *Rabochee dvizhenie v Rossii v period novogo revoliutsionnogo pod'ema 1910–1914 gg.* [The Workers' Movement in Russia in the Period of the New Revolutionary Upsurge, 1910–1914]. (Akademii Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 406. 2 r. 02 k.

In recent years Western and Soviet historians alike have convincingly documented the revival of the Russian labor movement and the emergence of the Bolshevik underground in St. Petersburg and other industrial centers on the eve of World War I. Much less certain are the reasons for the resurgent militancy as well as the relationship between striking workers and the Bolshevik cadres. Based on archival sources, including tsarist police reports, this account of the working class from 1910–14 by G. A. Arutiunov, a Soviet scholar, might be expected to provide concrete answers to these questions.

Unfortunately the book falls short. For example, Arutiunov describes the massive influx of poor

peasants into the urban areas during this period but then, without explanation, falls back on the standard assertion by Lenin that older, more experienced workers, imbued with the lessons of the 1905 Revolution, led the fight for political power. The reader suspects that the dramatic increase in strikes was in part a function of the semi-anarchic outlook of raw recruits from the countryside who were susceptible to appeals for direct action.

Nor is it persuasively shown—although the attempt is made—that Bolshevik leadership was principally responsible for the revolutionary upsurge. Quite the opposite appears to be the case: the success of Bolshevik slogans was due to the elemental fury of the workers at police repression and violence. Moreover, in one instance, in July 1914, the St. Petersburg Bolshevik Committee urged an end to city-wide demonstrations because the workers were out of control. Such allusions cast doubts on the author's use of sources, and we therefore await the definitive account of this seminal chapter in the history of the Russian revolutionary movement.

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ALEXANDER RABINOWITCH. *The Bolsheviks Come to Power: The Revolution of 1917 in Petrograd*. New York: W. W. Norton. 1976. Pp. xxxiii, 393. \$14.95.

With the defeat of the ill-timed leftist insurrection in July 1917, the Russian Revolution entered its most complex—and hitherto most dimly illuminated—period of development, the prelude to the establishment of the Bolshevik-led Soviet government. Alexander Rabinowitch of Indiana University, the author of an earlier study on the Petrograd Bolsheviks in the first six months of the revolution, has made a major contribution to our understanding of the events of 1917 by carrying his narrative through those crucial four months in which the party of Lenin emerged from isolation to become the effective spokesman for the workers and soldiers of the capital. The author's focus lies squarely on the party itself and on its most important leaders, Lenin, Trotsky, Kamenev, and Stalin, while the activities of other parties and the masses lie discretely in the background, contributing somewhat to the overall picture of social disintegration and radicalization of the lower classes.

Relying primarily on the contemporary press and published documents, Rabinowitch demonstrates convincingly that "the post-July days reaction against the left was not nearly as serious as originally feared" and that governmental repression helped "to increase resentment toward the Kerensky regime among the masses" (p. 313). In

an informative treatment of the *Kornilovshchina* the author deflates any notions that the Right could have successfully seized or held power when the soldiers, both at the front and in the Petrograd garrison, and workers in the city and on the railroads remained loyal either to the government or the Soviet.

With the defeat of Kornilov the possibility of a right-wing military dictatorship was eliminated for the immediate future, and the question of the nature of Russia's next government was placed once more on the agenda. The author clarifies the complex divisions among the active political forces in Petrograd on the question of power. The Leninists were adamant supporters, once again, of Soviet power. The Menshevik-Internationalists, Left Socialist Revolutionaries, and some moderate Bolsheviks favored a broadly-based, multi-party socialist government, while the majority Mensheviks and Right SRs favored continuation of some form of coalition with the bourgeois elements. The struggle for power by the Bolsheviks was simultaneously a struggle for the end of collaboration with the upper classes and an extension of the revolution into a socialist phase. The moderate socialists' seemingly suicidal support for a coalition government, even as the workers and soldiers abandoned such a concept, was based both on timidity and ideology, an unwillingness to shoulder alone the burdens of wartime government, and a misplaced loyalty to a notion of the "bourgeois revolution."

For nearly two months the coalition government attempted to find political forms and popular support with which to maintain itself. The decisive moment arrived when the Petrograd Soviet, now in Bolshevik hands, challenged the government's authority over the garrison. Rabinowitch gives us the best account to date of this struggle for the army. In the week leading up to the Bolshevik uprising, "the Military-Revolutionary Committee of the Petrograd Soviet took control of most Petrograd-based military units, in effect disarming the Provisional Government without a shot" (p. 314). Once this was accomplished, real power was in the hands of the Soviet; formal power followed with the fall of the Winter Palace and the ratification by the Congress of Soviets. It might be noted that workers seemed to have been conspicuously absent from the actual events of the October Days.

Historians have occasionally referred to social history as "history from the bottom up" and political history as "history from the top." The author provides us with a history from the middle, emphasizing party organizations, trade unions, soviets, and those leaders who stood between the government and the masses below. The Bolshevik Party is the key actor in this drama as presented, but the stage on which it performs is described

only in bare outline. The social structure of Petrograd, the composition of the work force, the economic and psychological pressures on the people of the city are dealt with scantily, if at all. Much more attention is given to the soldiers—and perhaps this is appropriate—but the impression with which one is left is that the buildup to the October Revolution occurred largely in committee rooms and on the pages of the partisan press. The role of the working class in this “first proletarian revolution” has still to be determined.

This emphasis on political organizations influences the author's brief discussion at the end of the volume of the reasons for the Bolshevik victory. Rabinowitch presents the intriguing “revisionist” thesis that Lenin's party hardly fits the stereotype of a highly disciplined and unified organization so often offered as the principal explanation of the Bolsheviks' remarkable success in 1917. Rather, he argues, “the party's internally relatively democratic, tolerant, and decentralized structure and method of operation, as well as its open and mass character” made possible a flexible, dynamic relationship between the party hierarchy and their potential supporters (p. 311). This argument is well defended by the close and detailed treatment of Bolshevik and Soviet conferences, committee meetings, and the press. Lenin was often at odds with his closest comrades, notably Zinoviev and Kamenev, and at times with the majority of his own Central Committee. Indeed, until the very eve of the Bolshevik insurrection, Lenin's desperate pleas for an armed seizure of power before the convening of the Second Congress of Soviets were largely ignored by Trotsky and the Military-Revolutionary Committee, which adopted a more moderate and defensive strategy against Kerensky and the Provisional Government. In the end it was Kerensky who initiated the military moves which precipitated the Soviet into launching the armed uprising.

The Revolution of 1917 will remain a subject to be plumbed by historians for a long time to come, but Rabinowitch's work has moved us closer to understanding the decline of the moderates in the Petrograd Soviet and the fall of the Provisional Government. While he does not adequately explore the social/historical setting of the events, his careful scholarship, balance and objectivity provide us with the single best account available of the intricate maneuverings of Lenin's comrades in their relentless and determined struggle for power.

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V. I. MILLER. *Soldatskie komitety russkoi armii v 1917 g. (Vozniknovenie i nachal'nyi period deiatel'nosti)* [Soldier

Committees in the Russian Army in 1917: Their Origin and the Beginning Period of Their Activity]. Moscow: Izdatel'stvo “Nauka.” 1974. Pp. 318. 1 r. 50 k.

Soviet scholarship on the Revolution of 1917 has heretofore given very little attention to soldiers' committees at the front, except for the few which were Bolshevik-dominated from the outset. Though there is a large fund of materials in the state military archives (TsGVIA) and a rich collection of army committee newspapers in the Lenin Library and elsewhere, the voluminous collections on 1917 contain but a small handful of useful committee documents; Soviet monographs seldom go beyond token reference to archival sources. Consequently, the genesis of the committees and their day-to-day operations have largely remained a closed book to Western historians. V. I. Miller has been a refreshing exception among Soviet scholars, first in a series of meticulously researched, truly innovative articles and now in this valuable book. It is chiefly due to his efforts that we now have a relatively complete account of the origins of Order No. One based on the record of the key Soviet session and other unpublished materials, as well as a richly documented account of the involvement of Guchkov, Stavka and the high command in the sponsorship of soldiers' committees. The intent of the command structure to take charge of this process to head off the spontaneous effect of Order No. One and the extension of the influence of the Petrograd Soviet to the front is no longer a matter of conjecture.

Miller also offers a relatively complete survey of the formation of higher level committees for each of the fourteen front armies, as well as of the major army congresses in March and April, though much of this information is available elsewhere. What is absolutely unique and cannot be duplicated by any non-Soviet scholar is the systematic profile analysis of seventeen regimental committees based on their written *protokoly*. Thirteen of the fourteen armies are represented and none of the well-known Bolshevik-dominated units is included, so that the sample can be regarded as reasonably typical. Miller makes the discriminations and observations that any good scholar would make, namely, that on this level the committees were far more concerned with the everyday concerns of their units (leaves, replacements, supply, soldier-officer conflicts) than with politics, that some were officer-dominated and utilized by the command and others took complete control, and that one nevertheless can discern a marked shift from loyalty to the Provisional Government and the notion of “victory” in March to loyalty to the Soviet and its peace program in April.

The study covers only the first two months of 1917, but one looks forward to one or more sequels. It might be argued that the process of committee formation from below is inadequately illuminated, and the impact of Order No. One is somewhat schematically handled, if not exaggerated, but sources on this question are elusive, even, apparently, in Soviet archives, and considerable room is left for speculative deduction. This is a very useful contribution to an area of knowledge that previously has not received its due.

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K. V. GUSEV. *Partiia eserov: ot melkoburzhuaznogo revoliutsionarizma k kontrrevoliutsii* [The SR Party: From Petit-bourgeois Revolutionism to Counter-revolution]. Moscow: Izdatel'stvo "Mysl'," 1975. Pp. 383. 1 r. 45 k.

Partiia eserov, K. V. Gusev's latest contribution to the study of revolutionary Populism in Russia, represents the first attempt in any language to cover the entire span of SR Party history in a single work. This is not to suggest that with this volume Gusev has undertaken a definitive history of the SR's. Rather, he offers us a collection of essays which can be divided chronologically into three periods: the first (three essays) describes the emergence of the SR's in the 1890s, as well as the growth and travails of the party before 1917; the second (eight essays) focuses on the turbulent revolutionary years of 1917-18; and the third (five essays) carries the story down to the garish SR trials of 1922.

Unfortunately, the sections of the book are uneven in quality. The essays on the earlier period suffer from the virtual non-existence of scholarly works on the SR's before 1917. Consequently, the fierce ideological struggles which rent the party from its inception are mentioned only briefly. Missing also is an account of the vast organizational-agitational labor carried out by the SR's before 1917, leaving unexplained the burgeoning strength of the SR Party after the February Revolution. The information on SR internationalism at home and abroad during the war, however, is fresh and provocative.

The shortcomings of the very detailed post-1918 essays are primarily of ideological provenance. To give one example, Oliver Radkey's recent study of the Tambov Green revolt has demonstrated the inadequacy of dismissing the numerous peasant risings of the period as mere "kulak-SR banditry" (Gusev, p. 343).

On the other hand, the 1917-18 essays, which ably and persuasively chronicle the growing isola-

tion of the SR leaders, their prodigal squandering of mass support, the consequent split in the party, and the subsequent growth of the Left SR's as a mass peasant-oriented movement, temporarily allying itself with the Bolsheviks in the name of Soviet power, are of interest even to the specialist. Here Gusev is in his element as he bolsters old arguments with new gleanings from archives and the latest Soviet studies.

With this lively, well-written book, Gusev has both pointed up the necessity for intensive study of the refractory and tantalizing historical problem of the rise and fall of the SR's, and suggested the outlines of a general history of the party. Perhaps the publication of this volume will spark interest in this neglected but quite important topic. Until further research is accomplished, *Partiia eserov* certainly occupies a valuable place in the historiography of the period.

MICHAEL MELANÇON
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OLIVER H. RADKEY. *The Unknown Civil War in Soviet Russia: A Study of the Green Movement in the Tambov Region, 1920-1921*. Stanford: Hoover Institution Press. 1976. Pp. xiv, 457. \$12.95.

It is hard to imagine that anyone will write a more detailed history of the Tambov peasant rising than this one. Oliver H. Radkey is a patient and conscientious scholar, who undoubtedly collected all the available material. He treats the peasants with deep sympathy, but at the same time there is no doubt about his scholarly integrity: he presents material which goes contrary to his theses. He takes the reader into his confidence by making explicit how he reaches his conclusions.

Yet this is a deeply disappointing book. First of all, the author fails to prove the importance of his subject. Antonov's twenty thousand peasant soldiers were a nuisance to the Bolsheviks, but not a genuine alternative to their rule. The peasants, who at the height of their success never occupied a single town, could not conceivably have established a government in Moscow, for they lacked the personnel, the means, and the ideology. We may deplore that we have to live in modern states, administered by large bureaucracies, but it is not within our power to change it. While the Civil War was in progress, the peasants could and did inflict great harm on their enemies, the Whites and the Reds, but they could not present a genuine alternative, a third way. Consequently, the title of this book is a misnomer: there was no unknown civil war in Russia, but a series of large and small scale peasant disturbances before the Bolsheviks finally consolidated their rule.

Secondly, there is not enough material to write

such a book as the author apparently envisioned. We learn very little about the life of the peasants, their organizations and world view. Even the hero of the story, Antonov, remains a shadowy figure. Radkey assures us that he was a great organizer and perhaps this is true: conclusive evidence, however, is missing. As a result, this long book is filled with trivia. The author spends pages discussing whether the little village Kliuchi was in one district or another and whether Antonov had twenty or thirty thousand soldiers.

Thirdly, Radkey shows no understanding of the enemies of the peasants, the Bolsheviks. One does not ask for impartiality; it is perfectly all right to side with the peasants. If he wants to explain what happened, however, he must give a more convincing description of Bolshevik behavior. According to him the Communists were simply evil men who hated the peasants and acted on the basis of their misconceived Marxist ideology. Radkey does not discuss the city dwellers who suffered even greater privations than the peasants. The Bolsheviks, in order to feed the workers, had no choice but to take food from the peasants. Radkey makes no pretense of impartiality. For example, he always refers to the Bolsheviks' leader, Antonov-Ovseenko, as "commissar of repression"—hardly a phrase suitable for a scholarly book.

Fourthly, Radkey's distaste for "intellectuals" makes it impossible for him to place the Antonov rising in proper perspective. He has expressed this bias in his previous books, but in this volume it is a major and constant theme. In his opinion the Greens had a better understanding of Communism than any intellectual. "Intellectual" is a notoriously vague concept and the spectrum, let us say, between Rosa Luxemburg and Oliver H. Radkey is very broad. I will continue to believe that at least some intellectuals had a greater political acumen than the illiterate peasants. The trouble is that this preconception does not allow Radkey to see the source of the real weakness of the movement which he describes. The peasants were condemned to defeat precisely because there were no "intellectuals" to help them to organize a functioning administration and to develop positive goals with which to win over the rest of the population.

Fifthly, the author has an irritating habit of polemicizing with Soviet historians. As a consequence he spends a great deal of energy in refuting ideas which hardly deserve to be noted. It is not necessary to prove that the Antonov rising was not organized and led exclusively by kulaks, or that Socialist Revolutionary politicians had no major role in it.

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LENNARD D. GERSON. *The Secret Police in Lenin's Russia*. Philadelphia: Temple University Press. 1976. Pp. xv, 332. \$15.00.

A book with a title as arresting as this one is guaranteed to attract attention, whatever its merit. It seems appropriate, therefore, to state at the outset that Lennard D. Gerson has investigated a politically sensitive subject with skill, admirable restraint, and impeccable scholarship.

The Soviet secret police became notorious under Joseph Stalin, but only in the past decade has serious research begun to penetrate the veil of secrecy surrounding the Stalinist police apparatus and to place it in the proper historical context. Alexander Solzhenitsyn's *Gulag Archipelago* recently brought the whole question of Soviet political repression before the general public. Others, including such scholars as Robert Conquest, Ronald Hingley, Robert M. Slusser, and Boris Levitsky, have focused more narrowly on the secret police itself. Gerson is the first to provide a reasonably comprehensive work on the Cheka (1917-22) and its offspring, the GPU and OGPU, with emphasis on Lenin's role as the major architect of the Soviet police state. The author's findings are based on a wide range of Soviet and Western sources, though archival material (other than selected tidbits quoted in Soviet secondary works) was obviously unavailable. The organization is topical instead of chronological, and while this arrangement was no doubt dictated by the nature of the subject, it does present difficulties. For example, frequent references to the Civil War, especially in the context of the Red Terror, are likely to confuse non-specialists with a plethora of unfamiliar names and events (but the more knowledgeable may wince at Admiral Kolchak's appearance as a "General"). Lengthy quotations from the sources and other pedantic idiosyncracies slow the pace of the narrative, but the style is brisk and uncluttered for so technical a monograph.

The author, perhaps correctly, saw his chief task as one of presenting factual information without editorial comment—indeed, without much interpretative analysis. Yet his distaste for the paranoid world of the Soviet secret police is conveyed by occasional flashes of irony and a conclusion that probes deeply if all too briefly into the nature of Soviet society. This is a valuable work—even "definitive" within its limits—which will not be superseded for many years.

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JACQUES LÉVESQUE. *L'URSS et la révolution cubaine*. (Travaux et recherches de science politique, number 42.) Montreal: Presses de la fondation nationale des sciences politiques. 1976. Pp. 219.

Jacques Lévesque of the University of Quebec proposes to fill a lacuna in the voluminous treatment of the Cuban Revolution by showing how it was perceived in Moscow after 1959 and how it posed new theoretical problems regarding the transition of Third World societies to socialism. His study is based on Soviet political literature, mainly the speeches of leaders, fortified by a wide perusal of periodicals and by interviews during visits to Cuba and the Soviet Union in 1973. Naturally, he is obliged to guess at the motives behind the various shifts in attitudes, but he found the literature far more revealing than he expected and often full of inconsistencies.

The Soviet Union suddenly became interested in Castro, whose rise to power had been aided little by Communists, as a means of fanning anti-United States sentiment in Latin America through the creation of a "national democracy" in Cuba. The ineffable Castro, however, proved difficult to control. Radicalizing at a far faster pace than Moscow favored, he provoked American intervention. Even after the Bay of Pigs and Castro's proclamation that he was a socialist, the Soviet leaders were reluctant to embrace him. They allowed themselves to invest in the installation of missiles in Cuba, to their humiliation. Again, they sought to restrain Castro, especially with regard to his rash plans for carrying the "armed struggle" to all of Latin America and for the rapid communization of Cuba. Moscow continued to subsidize Castro, but the years 1963-68 were marked by coolness.

The Soviet view proved sound. By 1969, most Communist parties in Latin America had broken with Castro, as had China; the American position was stronger; and Cuba reversed its economic plans to emphasize heavy exports of sugar. Cordiality between Cuba and the Soviet Union resumed, and by 1975 Castro was playing very nearly the role that Moscow had planned for him in 1960. This study stops before the invasion of Angola by Cuban forces.

Lévesque's work is clear and convincing. He is frank when he is merely speculating. If his discussions of theoretical problems in Marxism-Leninism raised by the Cuban Revolution are sometimes tedious to an unbeliever, the author has admirably discharged his self-imposed duty.

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NEAR EAST

MICHAEL AVI-YONAH, editor. *The World History of the Jewish People*. Volume 7, *The Herodian Period*. (First Series: Ancient Times.) New Brunswick: Rutgers University Press. 1975. Pp. xxii, 402. \$25.00.

This book is mistitled; it covers the history of the Jews in Palestine from Salome Alexandra (76-67 B.C.) to the eve of the revolt of A.D. 66. The editor has contributed an introduction on the rise of Rome and chapters on Jerusalem and on Palestinian Jewish art and architecture; A. Schalit, two chapters on the political history from Salome Alexandra to the accession of Herod; M. Stern, two on Herod and the history to 66; J. Klausner, H. Mantel, and S. Safrai, one each, on, respectively, Judean economy, high priesthood and sanhedrin, and temple and worship. Notes fill pages 339-85; bibliographies, 387-92; an index of names and places, 393-402. There are twelve maps and plans. After Avi-Yonah's death the editorial work was completed by Z. Baras.

The tone of all the chapters is set by Avi-Yonah's initial reference to the Jews as "the only people in the whole ancient world to have kept its identity" (p. 3). Copts, Greeks, Italians, etc., thus disposed of, the "historical" chapters report the whole course of events as a struggle of the Jews against Rome, in the purest "good guys vs. bad guys" tradition.

Within this tradition, however, there are various levels of competence. Avi-Yonah's introduction is with few exceptions a good factual summary. Schalit writes from much knowledge of Herod's times and many untenable assumptions: e.g. the people must always follow the Pharisees, oppose the Romans, and desire independence; so they support Hyrcanus against Aristobulus (p. 29), Aristobulus against Hyrcanus (p. 41), and also the opponents of both (p. 31). Equally bad are the accounts of Gabinus' reforms (pp. 40-45), the large settlement of Roman "colonists" in Jerusalem (in 57 B.C.! p. 40, cp. 42), the trial of Herod before the sanhedrin—of which Josephus, in the *War*, knew nothing—(pp. 48ff.), and so on. Stern's chapters are much better; his account of the status of the province of Judea and its governors is outstanding (though few will accept his evaluation of the reign of Agrippa I as "the last golden age of the Jews in antiquity" [p. 139, cp. 148]. What of rabbinic literature?). Poor Klausner's notes—a jumble of data on all periods from 1000 B.C. to A.D. 600—should not have been exhumed. Avi-Yonah's chapter on Jerusalem is a remarkably clear account of a gigantic jumble—indeed, somewhat clearer than the facts; on Jewish art and architecture he is beautifully precise, but does not mention that the pious Agrippa I had statues of his daughters in his palace at Caesarea. "Symbolic values" are not mentioned either. Mantel's description of high priesthood and sanhedrin and Safrai's of temple and worship, both vitiated by efforts to square first-century evidence with second-century and later rabbinic theory, are mainly of interest as exercises in piety and *pilpul* (the

Talmudic art of explaining away awkward facts by facile assumptions).

The pictures are well chosen, mostly good, some excellent. The Greek quotations swarm with errors. The book is big, heavy, and hard to handle, in sum, monumentally uninspiring.

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GEOFFREY HINDLEY. *Saladin*. New York: Barnes and Noble. 1976. Pp. xv, 208. \$18.00.

This latest contribution to the literature on Saladin, like most of its predecessors, is aimed at a popular rather than a scholarly audience. As such, it is not without merit. To begin with, it is effectively organized; by opening with Saladin's conquest of Jerusalem, Geoffrey Hindley lends a sense of direction and high purpose to the tortuous maneuverings of the sultan's first two decades in power. Moreover, the author's writing, though often careless, is generally clear and attractive. On a different level, he has a fairly sound grasp of Saladin's place within the political history of the twelfth-century Islamic East, and he shows a real awareness of the constraints imposed on Saladin's policy by his dependence on his kinsmen. There are shortcomings in these areas, to be sure, but given Hindley's decision to focus on the personal achievement of Saladin, they do not seriously compromise his book. Finally, the author must be credited with several shrewd insights into the goals and motives of his characters.

Having noted these virtues, I must stress that this book will be of little value to scholars. First, the author knows no Arabic. Moreover, his bibliography and notes display some astonishing gaps. The standard surveys of the Crusades by Setton, Prawer, Mayer, and Stevenson are not to be found, nor is Elisséeff's *Nur ad-Din*. Even the name of the author's *bête noire* Ehrenkreutz is consistently misspelled. Second, there are many errors of fact. Some of these are merely careless, as when he calls al-Qadi al-Fadil a Kurd. But others betray a deeper lack of understanding, as when he tries to explain the hostility of the Nuriyya amirs (whom he dubs Nurriayahs) to Saladin; these amirs were not so named because of their partisanship, of course, but because they were officers of Nur ad-Din's personal regiment. In short, the book cannot be relied upon even as a digest of the scholarly literature.

Perhaps the most important shortcoming of the book is its conventional approach to its subject. Plainly the author intends a vindication of Saladin's idealism and leadership against the recent onslaught of Ehrenkreutz. In his account of the Reconquest and the Third Crusade (by far the

weakest section of Ehrenkreutz' work) he makes many effective points, but he is less successful as regards Saladin's earlier years. Here he is compelled to accept many key elements of his adversary's indictment, and can only apologize for Saladin's persistent opportunism and treachery by pointing to the higher cause which they served. But he quite ignores the real substance of Ehrenkreutz' attack—that Saladin's wars nearly ruined the already weak economies of Syria and Egypt. It is this aspect of Saladin's reign which should demand research henceforth; of his inmost thoughts we can know very little, while there is much to be learned about his impact on the political, economic, and social structures of the age.

R. STEPHEN HUMPHREYS
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ABDO I. BAAKLINI. *Legislative and Political Development: Lebanon, 1842-1972*. (Consortium for Comparative Legislative Studies, number 2.) Durham, N.C.: Duke University Press. 1976. Pp. xv, 316. \$11.50.

Those who have written on Lebanon, Abdo I. Baaklini believes, have been led into gross misunderstanding by a mistaken emphasis on sectarianism and by the notions, common among contemporary theorists of development and modernization, that a strong executive is the source of modernization and that a democratically elected legislature is necessarily dominated by traditional elites and thus anti-modernizing. After briefly surveying Lebanese roots in the eighteenth and nineteenth centuries and the creation of the state during the French Mandate, the author undertakes a systematic analysis of Lebanese government since the National Pact of 1943. He treats ideologies and the laws and regulations which govern elections, the parliament, the cabinet, and the presidency. Dynamics and functions are investigated by means of statistical analyses of parliamentary membership and recruitment patterns and of voting behavior. In all these areas the book contains a great deal of useful information. Nevertheless, the author's main conclusions do not seem entirely satisfactory. Baaklini argues, correctly I believe, that confessionalism in politics means competition within individual religious communities, not competition among them, and that the system indeed promotes cooperation across confessional lines. He also argues persuasively that the Lebanese parliamentary system has facilitated peaceful change and development. Both these happy conditions have been the rule in Lebanon throughout most of the country's history. But there obviously is an unresolved tension.

Baaklini does not explain, in fact he ignores, two

salient facts about independent Lebanon. Firstly, violence has played a major role in three out of five presidential successions. Secondly, modern Lebanon has twice been torn by civil war of a very peculiar nature; neither disturbance was in the usual sense a conflict of Moslem against Christian, as is commonly and incorrectly stated, for the political leadership did not split along religious lines; but nevertheless the actual combat was to a very large extent a case of Moslems against Christians. This paradox neither Baaklini nor anyone else has yet begun to explain. In short, Baaklini has shown how well the Lebanese system has worked most of the time, even though he offers no explanation for the periodic approaches to collapse which also appear to be characteristic of Lebanon.

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ROBERT MABRO and SAMIR RADWAN. *The Industrialization of Egypt, 1939-1973: Policy and Performance*. New York: Oxford University Press. 1976. Pp. 279. \$14.75.

This is an outstanding example of the application of econometric techniques to the study of history. If the final product is not quite cliometrics, it is only because the authors are at heart economists, not historians. Their penchant for quantification and their desire to find statistical answers to their problems often cause them to ignore questions historians would consider worthwhile. The task the authors set for themselves is to describe Egyptian industrial development during the years 1939 to 1973—a period when industry was the fastest growing economic sector. Using Egyptian government publications with skill and appropriate circumspection, they create a portrait of the Egyptian industrial economy which is statistically fuller and more precise than previous studies. Although their treatment of nineteenth-century efforts at industrialization does not offer anything novel, once they examine the 1950s and 1960s they have a number of interesting points to make. To mention but a few, their discussion of Egyptian tariffs is important. Many scholars have believed that the introduction of tariff reform in 1930 ushered in an era of high tariff protection and sparked industrial development. While not disputing the importance of tariffs for industrial development, Robert Mabro and Samir Radwan point out that tariffs in the 1930s were not particularly high. Indeed, they could not be exclusively protectionist because customs were the primary source of state revenues. They did not become a protective wall behind which industrial development took place until the 1950s. Mabro and Radwan's discussion of Egypt's import sub-

stitution policy is also wise and sensible. They point out that import substitution can be a misnomer, as it was in Egypt, because industries created to manufacture previously imported commodities often rely on a considerable quantity of imports. The Egyptian textile and food processing industries did indeed replace imports. But they in turn became large importers of raw materials and intermediate goods.

In their conclusion the authors insist that their work should not be seen as a critique of all developmental policies based on state planning and publicly controlled enterprises. Yet their study, taken in conjunction with other recent and critical studies of the Egyptian economy under Nasser, offers a severe indictment of this one particular state-run economic system. The authors are critical of the state pricing policies which enabled inefficient producers to stay in business. They condemn labor policies which made it difficult for business firms to fire employees. They believe that stripping managers of public enterprises of their freedom of operation led to poorly run firms. In short, it would be difficult for a reader not to conclude that the Egyptian economy today needs a dose of liberalization—a conclusion that has been echoed by most recent economic studies and various international organizations and has been accepted in theory by the Sadat regime.

An important criticism of this book is that connections between economy and polity and society are not made. The study is rigidly and almost aridly economic. Mabro and Radwan deal with dramatic economic changes—a move from *laissez-faire* free enterprise to a mixed economy, and then to a state-run system in the 1960s. The reader learns a little about the economic factors that lay behind these changes, although by and large the authors' point is that economics did not compel such changes; but he gains little appreciation of the political, ideological, and personal factors that moved Egyptian affairs steadily in a statist direction. The Egyptian economy cannot be so neatly separated from its polity and society nor can economic statistics and policy be treated as though they were disembodied phenomena.

ROBERT L. TIGNOR
Princeton University

HOWARD M. SACHAR. *A History of Israel: From the Rise of Zionism to Our Time*. New York: Alfred A. Knopf. 1976. Pp. xviii, 883, xlix. \$20.00.

This massive and excellent volume treats the history of Israel within the context of her intricate relationships with world Jewry, the Arab world, the United States and other Western powers, and Soviet Russia. It also contains detailed analyses of

the internal developments in Israel, and its remarkable achievements in the economic, social, scientific, intellectual, artistic, and other cultural fields. It is studded with an abundance of statistical data covering politics, demography, immigration, living standards, agriculture, industry, health, etc. At the same time it has a broad historical perspective and manifests an accurate discernment of the major trends in the overall growth processes of the young state.

We are shown Israel's timid beginnings in the late nineteenth century and the successive stages in its evolution which led to its position in the 1970s as the cultural center of world Jewry, the spearhead of Western democracy in the Middle East, the bulwark against Soviet domination of the area, and a major force to reckon with in the Eastern Mediterranean. All in all, this book in both conception and execution, the sweep of its presentation, its keen analysis and sensitive interpretation, bears the hallmarks of a magnum opus. In addition, the author has a talent not often found in modern historians: he can tell his story interestingly. I found Sachar's book fascinating reading.

What comes through clearly in the author's presentation of the history of Israel, and especially in the last decade, is the extraordinary measure of Israeli dependence on global power relations. Israel could win wars against the Arab states, yet the fruits of victory all but eluded her because she was not in a position to resist pressures from the United States. The U.S., on its part, while a staunch ally of Israel (in fact the only such ally after the French and British cave-in in the face of Arab oil-power), could not but regard Israel from the viewpoint of its own global strategic and diplomatic interests. Thus, the security of Israel, Sachar shows, depends as much on the relationship between the U.S. and the USSR as it does on the power of her own arms. A small country like Israel simply seems to have no alternative but to remain in the good graces of the American superpower, and to make sure that neither Israel itself nor the U.S. loses sight of the basic identity of interests that have developed between the two.

Such a fine book will undoubtedly go into second and subsequent editions. If so, the author would be well advised to check and eliminate the occasional spelling errors in proper names and some other minor factual mistakes. Mention must be made of the numerous clear battle maps and the excellent analytical fifty-page index.

RAPHAEL PATAI
Forest Hills, New York

RONALD M. DEVORE. *The Arab-Israeli Conflict: A Historical, Political, Social, and Military Bibliography*.

Santa Barbara, Calif.: Clio Books. 1976. Pp. xxxiv, 273. \$17.50.

This work forms part of a bibliographic series published by the Center for the Study of Armament and Disarmament, California State University, Los Angeles. The bibliography includes over three thousand titles of books and articles published up to 1974. Apart from a few items in French, only English-language titles are included. Entries are classified under a number of subject sections, and there is a useful index. The book is marred by a superficial introductory essay in which the editor seeks in eleven pages to encapsulate the history of the Arab-Israel conflict from its beginning to Kissinger. A brief chronology from A.D. 70 to 1974 contains curiously contrasting references to "Zionist terrorists" and Palestinian "suicide attacks." The editor is at pains to stress that the bibliography is "not definitive" but is intended as a "working tool" (p. xxxiv). The disclaimer is prudent as several standard works of central importance to the subject have been omitted. Specialists will have little use for this book since it excludes all important work not available in English. Non-specialists may find it of limited value, but are in danger of drowning in a sea of superannuated propaganda.

B. M. J. WASSERSTEIN
University of Sheffield

SABRI JIRYIS. *The Arabs in Israel*. Translated by INEA BUSHNAQ. Foreword by NOAM CHOMSKY. London: Monthly Review Press. 1976. Pp. xviii, 314. \$12.50.

Sabri Jiryis, a graduate of the Hebrew University of Jerusalem, was an Israeli Arab lawyer and political activist who left in 1970 for Beirut, where he became prominent in the Palestine Liberation Organization. In 1976 he tried to start a PLO office in Washington, D.C. His book, first published in Hebrew in 1966 in a smaller and partially censored version, has since been expanded and translated. This, the first English edition, is published by a Marxist publishing house and has a lengthy foreword by Noam Chomsky condemning Israel as pseudodemocratic and its American supporters as deluded.

Jiryis focuses on the question of personal and property rights of Arabs in the pre-1967 borders of Israel, the problems they encountered, and in consequence, their presumed victimization, then resistance. His study is, intellectually, a definite step above conventional Arab anti-Israel propaganda. It also has the attribute of documentation (though no index), chiefly from the Hebrew press, official state compendia, Knesset debates, and memoirs. The work is, however, much more advocacy journalism than history. Besides the scarcely muted

PLO party line toward "colonialist Zionist imperialism" (p. 131), the facts-in-the-round are rarely given.

Still, historians will find the twenty-five pages of statistical tables and notes on the Arab minority and also chapter seven, which deals with Jewish-Arab politics within the Mapai, Mapam, and Israel Communist parties, useful and credible. Interesting, detailed, but slanted are the chapters (1-5) describing Arab grievances against Israeli martial law, security regulations, and Knesset legislation on expropriated Arab land and absentee ownership.

Jiryis' work points out Israel's perennial, perhaps impossible, internal challenge: to make Israel a distinctly Jewish state and also an open secular society where non-Jews neither are nor feel as second-class citizens. It is interesting that the author himself, though a PLO moderate regarding possible acceptance of a small Israeli state (pp. xiv, 192, 239), affirms that "struggle" and "armed resistance" (pp. 237-38) are the only road the Arabs should take against Israel.

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AFRICA

RICHARD GRAY, editor. *The Cambridge History of Africa*. Volume 4, *From c. 1600 to c. 1790*. New York: Cambridge University Press. 1975. Pp. xiv, 738. \$42.50.

In 1962 Roland Oliver and John Fage brought out their *Short History of Africa*. With a large exclamation point it punctuated the postwar movement, to which they had contributed with great distinction, toward the establishment of systematic studies of African civilizations past and present. More particularly it demonstrated to a worldwide readership that Africans had their own history, that this was a history with its own integrity and not a mere appendage to the story of European commercial expansion or colonial administration.

At the time of its publication, the *Short History* was regarded as a sort of outline sketch of Africa's past which the two collaborators would eventually expand into a full-scale, possibly multi-volume, history. Its broad-ranging, overarching themes certainly lent themselves to this understanding. They gave shape to the African past and meaning to its many events. If some of these themes later came into question, that scarcely reduced their value or compromised their validity; revision is, after all, the normal fate of all historical interpretations.

Alas, Fage and Oliver never wrote their history, but they have edited the new *Cambridge History of*

Africa, of which the fourth volume, under the editorship of Richard Gray, is the first to appear. In many ways it is a mettlesome work—over seven hundred pages crammed with data painstakingly gathered and presented by knowledgeable and acknowledged experts. There are eight lengthy chapters covering various geographic areas from the Maghrib to Madagascar with a final chapter devoted to Africans abroad, largely in terms of the slave trade. Useful historiographical essays are available for each chapter along with selected bibliographies of available secondary works. Much of the information in the text is drawn from previously published works, but there is some new material along with the sure-handed exposition of experienced professional historians.

There is also much that is missing. To begin with, the contributors are narrowly chosen. Virtually all have been associated with London University's School of Oriental and African Studies. This may provide a useful unity of approach, but it also suggests a degree of inbreeding which is disturbing. No doubt this difficulty may be rectified in future volumes. Doubtless, too, such a concentration reflects the solid achievements of S.O.A.S. over the years in its programs of teaching and research on Africa. One would hope it does not also reflect a sense of orthodoxy and possessiveness. Particularly unfortunate in this respect is the absence of any African scholar.

Even more disturbing is the character, or more particularly, the lack of character, of the volume. Somehow it has become the antithesis of Oliver and Fage's *Short History*. Nothing stands out. No themes emerge. Where are the unities? What is the shape? Why was it written? Every page assaults the reader with its detail; all recite their erudition but few explain their meaning. To take one example, there are innumerable references to migrations of the great and small, but no word concerning the epic movements of Nilotes or Bantu-speaking peoples which would provide some overall understanding. Again, long-range developments like the expansion of the Boers or the interaction of Nilotes and Bantu are treated in a two-hundred-year slice with little hint of what came before or after.

Part of the difficulty surely lies in the organization. There seems to be an almost perverse insistence by British-trained Africanists on dividing their subject into temporal and regional units. Robin Hallett crippled his massive history with such an arrangement, and now we find the same error in the Cambridge history. If separate chapters on Ethiopia and Egypt make some sense, why, for example, must the western and central Sudan be divorced while the western savanna is linked to the Maghrib? And why the period 1600 to 1790? One would expect from such distinguished editors an organization based upon organic unities—for

the Sudan, perhaps the trans-Saharan trade or the press of environment in giving shape to society. As for the time span, one has the giddy feeling that, in planning the whole series, the editors somehow found themselves with a gap, 1600–1790, and a volume of seven hundred blank pages that had to be filled with something. Richard Gray's introduction seems to hint as much.

Perhaps most of the trouble lies with our modern universities, staffed by specialists busily training little specialists who will eventually issue forth to cultivate their own little specialties. Everyone is so bent upon exhuming new facts, they have no time to make distinctions between the important and the trivial and no apparent standards for making such distinctions. Indeed, there is small evidence that they have ever been trained to think in terms of priorities, to ask what purpose they serve, what principle they describe, what truth they seek.

It may be argued that a reference work is supposed to get the facts down and never mind the interpretations. This the *Cambridge History* does admirably, and for students of African history it may be sufficient, although they would already know how to obtain most of the same material elsewhere. But what about the general reader? How is he supposed to make sense out of the massive weight of information that bears down on him? He will certainly have difficulty with the *Cambridge History of Africa*, at least as exemplified by this initial volume.

ROBERT W. JULY
Hunter College

CHRISTOPHER CLAPHAM. *Liberia and Sierra Leone: An Essay in Comparative Politics*. (African Studies Series, number 20.) New York: Cambridge University Press. 1976. Pp. vii, 156. \$10.95.

MARTIN LOWENKOPF. *Politics in Liberia: The Conservative Road to Development*. Stanford, Calif.: Hoover Institution Press. 1976. Pp. viii, 237. \$9.95.

Liberia and Sierra Leone adjoin, divided by an arbitrary frontier. Their indigenous peoples have much in common, not least that each has had immigrant populations planted on them—the Americo-Liberians and the Sierra Leone Creoles. In other ways they have differed. Sierra Leone experienced British colonial rule. Liberia has been independent since 1847. But the similarities are great enough to warrant comparison.

Christopher Clapham has done more than compare them. His short, illuminating book also makes an impressive contribution to the study of comparative politics. His approach is descriptive, not explanatory. No models are constructed: he does not believe in a science of comparative poli-

tics. Following a line of approach suggested by F. G. Bailey's seminal *Stratagems and Spoils* (1969), he interprets politics as a regulated struggle for benefits, in which politicians make use of the available political resources by means of recognized rules, illustrating with refreshing clarity how they do so in the two countries.

As well as using his own perceptive fieldwork and documentary research, Clapham has been able to draw on two comprehensive studies of twentieth-century Sierra Leone—Martin Kilson's *Political Change in a West African State* (1966), an important pioneer work covering the colonial period, and John Cartwright's *Politics in Sierra Leone 1947–67* (1970), a more detailed account with a narrower focus—and a section in A. Bebler's *Military Rule in West Africa* (1973). He has used two unpublished doctoral theses on local politics—Walter Barrows' on the Kenema District, now published as *Grassroots Politics in an African State* (1976), and Victor Minikin's "Local Politics in Kono District"—and unpublished work by Roger Tangri. Neil Leighton's thesis on the Lebanese, also used, has since been supplemented by H. L. van der Laan's magisterial *The Lebanese Trader in Sierra Leone* (1975).

Less has been published on Liberia. Historians have the valuable *Liberian Studies Journal*, now in its sixth year, edited by Svend Holsoe, but the only comprehensive political study, Gus Liebenow's *Liberia: The Evolution of Privilege* (1969), well-informed though it is, treats the country in a light-hearted way that no American or European scholar would have used in studying Ghana or Guinea. Robert Clower, too, struck a censorious note in the collaborative *Growth without Development: An Economic Survey of Liberia* (1966). Clapham's descriptive approach is more appropriate to an era when it is less easy to believe that American and European scholars know all the answers than it was ten years ago.

Eschewing praise or blame, he begins with the political resources of the two countries and finds them very similar. In both the populations of immigrant and indigenous origin have in different ways reached a *modus vivendi*. Both are faction ridden. In both wealth is a prize, generated by foreigners, to be competed for. It is not a resource to be created. In both status goes with political power and education.

Where they differ is in their political rules. Liberia has for generations supported a stable, homegrown political system. The rules were made by the Americo-Liberians, to maintain themselves in power, but have gradually been adapted to extend rewards more widely. Politics are centralized, rival factions struggling within one coherent system. Everyone understands the rules.

In Sierra Leone the colonial government left a legacy of disunity. Local groupings, usually perceived as "tribal", form an important focus of loyalty. Aspiring politicians tend to establish themselves in local power bases, maintaining themselves by alliances with other factional leaders. There is no single way up. Political life is uncertain, as witnessed by the constant coups and attempted coups since independence. Hence individuals or factions are tempted to seize power and make their own rules, and governments must rely on openly coercive methods to maintain themselves. In Liberia, where power struggles are structured within an accepted framework of unobtrusively coercive rules, open coercion is rare.

Clapham goes on to illustrate differences of emphasis in the relations of "centre and periphery" (still a favored Castor and Pollux among political scientists). In Sierra Leone the office of Paramount Chief is still a valuable prize which may give potential leverage at a national level. In Liberia it is normally only a stepping stone toward office at the center. There all roads lead up to the president, who descends regularly on the countryside, where he and the other national dignitaries maintain country estates, to intervene in Olympian style in local affairs. In Sierra Leone, where the land tenure system prevents individuals from owning estates in the interior, presidents and prime ministers tend to stay in Freetown. The Liberian system makes for peace and quiet, the Sierra Leonean for simmering conflict.

Finally he examines the economies of the two countries—not as influencing their political systems, but as political systems in themselves. In both foreigners create wealth and governments try to prevent them from keeping it all—for instance, by restricting certain types of trading to citizens of the country (this usually ends up giving politicians access to more rewards), and by extracting more revenue from the mining companies. The politics of agriculture enables governments to extract revenue from farmers through marketing boards. In Liberia politicians often invest their gains in agriculture. In Sierra Leone, with its restrictive land tenure system, they do not. In general, Sierra Leonean politicians, harrassed by factional rivalries, have been less able to exploit the economy to their own advantage than the Liberian. Even so, Clapham finds the similarities between the two systems outweigh the differences.

Plainly the comparison has proved most rewarding. Clapham has described what is actually going on in Liberia and Sierra Leone—as distinct from what, had he been a different type of political scientist, was going on inside his own head. He has also given a constructive, lucid demonstration of how to make fruitful political comparisons.

He mentions having used one unpublished work on Liberia, a doctoral thesis by Martin Lowenkopf, now published as *Politics in Liberia*. Based on research done in the 1960s, it is a study of Liberia during the Tubman era. What gives the book an outdated look is not the material (which historians will find useful), but the approach. Unlike Clapham, Lowenkopf is concerned not just to describe but to explain, adopting as his explanatory tool the worn out concept of "modernization." Fashionable during the early 1960s (Kilson used it in his study of Sierra Leone), it has now proved too question-begging, too value-laden, to be permanently helpful to students of African politics. Time has therefore robbed this book of the acclaim it might once have won.

CHRISTOPHER FYFE
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RAJESHWAR DAYAL. *Mission for Hammarskjöld: The Congo Crisis*. Princeton: Princeton University Press. 1976. Pp. x, 335. \$17.50.

It is ironic that so many important people were participants in the events recorded by Rajeswar Dayal in *Mission for Hammarskjöld: The Congo Crisis*. Dayal, the special representative of Secretary Hammarskjöld's U.N. Mission to the Congo, 1960–1962, writes a brilliant and fascinating account of the largest operation in the history of the United Nations. Eighteen chapters, well organized and supported by an inclusive bibliography and selected illustrations, portray an epoch of unusual significance. Belgian African effort is revealed. The rich natural resources of the Congo are discussed, and the determination of tribal groups to form a nation state is explained. Moreover, it is significant that the pre-1960 riots in Leopoldville are examined. Essential training and experience denied the Congo by Belgium caused great problems and a world crisis. Premier Moïse Tshombe of Katanga had long sought ways to achieve separatism in the heart of Central Africa.

Independence for the Belgian Congo produced a vast gap between promise and fulfillment. Except for a few Congolese politicians, July 4, 1960 did not mean Africanization. It was the beginning of violence that forced the Security Council to provide military forces.

African and Asian countries helped in keeping the area contained. Cobalt, diamonds, uranium, tin, copper, timber, palm-oil, cotton, coffee, rubber, and immense reserves of hydro-electric power as well as the integrity and protection of millions of natives made the stakes high. Lumumba, well-intentioned but ill-directed, complicated the mission. Abnormal conditions had to be corrected—and time was precious. Hammarskjöld regarded

the military strength of the U.N. as largely symbolic; very important, when one considered the American and British partisan support of the Belgian position.

Colonel Joseph Ankrah attempted to establish a rapprochement, but the disunited nation grew more suspicious. Moreover, there was a dichotomy between public utterances and private assurances. Pan Africanism was omnipresent and optimism was not apparent.

Several core chapters explain the success and failure of the mission, and chapters 17 (Lumumba: Dilemma and Reality) and 18 (Hammar skjold: Self Realization) reveal the inner thoughts of Lumumba, "the Messiah of his people," and Hammar skjold, "the peace maker with undying faith." Both men, although with different aims, played leading roles in the Belgian Congo drama.

Dayal's *Mission of Hammar skjold* is recommended for the public interested in African and world peace. It is a well-written hour-by-hour account of the daily routine of the search for stability. This "inside story" is readable and informative. "Hammar skjold, who always tried to rise from the particular to the impersonal was pitted against Lumumba, in whom impersonal love of country was metamorphosed into a sense of personal dignity" (p. 309).

The author has secured Hammar skjold's place in history. Yet, many other leaders are also brought into the story, and their roles, so ably explained by Dayal, will allow the reader to better understand the complicated endeavors for peace in Katanga and the Congo. Strangely, Lumumba and Hammar skjold, each totally dedicated to his course, both lost their lives, but perhaps succeeded in their mission in the Congo.

ALONZO T. STEPHENS
Tennessee State University

ALBERT WIRZ. *Vom Sklavenhandel zum kolonialen Handel: Wirtschaftsräume und Wirtschaftsformen in Kamerun vor 1914*. (Beiträge zur Kolonial- und Überseegeschichte, number 10.) Zurich: Atlantis. 1972. Pp. 301. 39 S. fr.

This welcome addition to the growing research on Cameroon economic history deals with the effects of European trade on various Cameroon societies. It complements the earlier metropole-centered study by Hausen in the same series.

After introductory chapters on theories of "primitive economics" and German colonial policy, Albert Wirz describes the socio-economic structure of three major areas: the coastal Duala, the forest zone of the south, and Adamawa Emirate in the north. He begins by analyzing the shift from slave trade to legitimate trade on the coast

and the resultant changing political structure of the Duala middlemen. The shift had an egalitarian and disruptive influence which accounts in large part for the Duala chiefs' eagerness to sign protectorate treaties with the Germans in 1884. The chiefs' desire to gain outside support for the preservation of the status quo converged with growing German interest in the scramble for Africa. By not considering data from the period of the slave trade, Wirz prevents the full impact of three centuries of slaving from manifesting itself in his analysis. Research now in progress on Duala history will revise and expand some of Wirz's views. From their foothold on the coast, German traders expanded into the south-central forest zone. The author describes the precolonial ivory trade, the destructive German trade in rubber, and the introduction of cash crops.

Wirz's description of the precolonial and colonial economic network in Adamawa is perhaps the most valuable part of the book. He traces the expansion of the Hausa caravan network into Adamawa, a distant province of the Nigerian Sokoto Caliphate, and south all the way to the coast at the beginning of this century. Thus north and south were linked, under German aegis but through mostly African initiative. Wirz's utilization of the colonial archives in East Germany makes this chapter an important contribution to Adamawa history.

The research is thorough and meticulous but restricted to published and archival material. It is unfortunate that the author was not able to turn his obvious talents to the collection of oral materials, to better balance his sources. For completeness it would have been preferable to include the better known German plantation economy around Mt. Cameroon, but this was precluded apparently by the inaccessibility of the Buea archives, a condition now remedied. One hopes that this fine work will be translated into either French or English, to reach a wider African audience.

RONALD RAPHAEL
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Bloomington

R. W. BEACHEY. *A Collection of Documents on the Slave Trade of Eastern Africa*. New York: Barnes and Noble. 1976. Pp. xii, 140. \$11.00.

R. W. BEACHEY. *The Slave Trade of Eastern Africa*. New York: Barnes and Noble. 1976. Pp. 324. \$18.00.

It was true in 1967, as Edward Alpers observed in his pamphlet *The East African Slave Trade*, that the slave trade of eastern Africa had "attracted much less attention from both professional historians

and popular writers" than had the Atlantic slave trade. Until the present decade, moreover, scholarship concerning the East African traffic was dominated by the work of the British imperial historian, Sir Reginald Coupland. The last ten years, though, have seen considerable research and writing on this subject. Far from the "blanket of silence" that R. W. Beachey describes in the preface to his work, this scholarship has served to illuminate the commerce in human beings as a part of East African, as well as European and Middle Eastern history.

Beachey's effort attempts to provide, in one volume, selections of documents on, and in another volume, a thorough analysis of, the trade in all areas of eastern Africa, from Delagoa Bay to Suez. The work fails, however, to achieve any significant synthesis, nor does it offer substantial new information. It is not, in fact, a treatment of Africans and the slave trade. Rather, it is a consideration of the European and Arabian trade in African slaves and of British efforts to suppress the traffic. In this, Beachey only offers more of the imperial perspectives of Coupland, first developed more than forty years ago. Even the attempt "to establish an overall figure for slave exports from eastern Africa in the nineteenth century" is little more than a sop to recent interest in quantitative history and lacks any of the rigor expected of such study.

Beachey's treatment of the slave trade is far too narrow, lacking the depth and breadth of vision which is necessary to understand its role in the East African, or an even wider, context. As an institution, slavery and the slave trade had important economic and social dimensions, offering Africans new elements of choice and control over their own lives. Likewise, Africans were able to exploit the demand for slaves, as well as for ivory and other commodities, to their own political advantage. For example, the Nyamwezi leader, Mirambo, was able, by controlling the flow of trade through central Tanzania, to enhance his own position and build a powerful empire. Yet Norman R. Bennett's thoughtful book on this skillful African is ignored by Beachey, and Mirambo himself is dismissed in a scant page. Similar treatment is afforded almost all the recent studies on African aspects of the slave trade.

Such shortcomings are magnified by Beachey's obvious bias. His collection of documents is dominated by the observations of British naval and consular officials, missionaries, and antislavery crusaders. These very same individuals animate the analytical volume, almost to the exclusion of French, Portuguese, and German observers. There is much taken from the *Anti-Slavery Reporter*, but nothing from its counterpart, *Mouvement Antiesclavagiste*; archival sources do not go beyond Brit-

ish parliamentary and foreign office papers. Arabic materials, even in translation, are notably absent from the notes and bibliography. One suspects that this singlemindedness may not have been wholly unintentional, for Beachey clearly brings an English abolitionist perspective to his task. This prejudice is evident in his description of Sir Arthur Hardinge, British consul at Zanzibar during the last days of slavery on the island. "Few British officials," Beachey laments, "could have been less English in their approach to the question of slavery" (p. 242).

In short, there is little to suggest that Beachey's books will fulfill the publisher's dust-jacket claim that they "should become the standard work on the subject." They belong, instead, to a historiographic tradition long past and as such their current value is gravely diminished.

MELVIN E. PAGE

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ALLEN F. ISAACMAN: *The Tradition of Resistance in Mozambique: The Zambezi Valley, 1850-1921*. In collaboration with BARBARA ISAACMAN. (Perspectives on Southern Africa, number 18.) Berkeley and Los Angeles: University of California Press. 1977. Pp. xxiv. 232. \$13.75.

Allen F. Isaacman presents an analytical and comparative discussion of the forms, goals, strategies, and historical links of African responses to Portuguese colonialism in Mozambique from 1850 to 1921. He points out that from 1550 to 1921 Portuguese colonialism was characterized first by trade expeditions, administrative impotence, and interference in the internal affairs of African societies; then by the *prazo* system and the arming and encouragement of secondary conquest states such as Gorongosa; and, finally, by conquest and colonial administration.

By 1700, Portuguese colonialism had caused the decline of the Mutapa Empire and led to political reorganization and rehabilitation throughout the Zambezi Valley, characterized by African expansionism, rivalries, dynastic factionalism, and new conquest states, and worsened by Nguni migrations and the slave trade.

Isaacman reveals a long tradition of African resistance to colonial domination (European or African) in both the precolonial and colonial periods. With respect to Portuguese conquest, the Mutapa, Barwe, conquest states, and several smaller polities waged some impressive resistance, but by 1902 they all had been defeated, as a result of succession crises, old rivalries, ethnicity, espionage, ineffective alliances, and inferior weaponry. The Portuguese were able effectively to divide and conquer, outgun, and, surprisingly, outnumber Afri-

cans by recruiting mercenaries, especially among the Ngoni.

From 1902 to 1921 Africans were responding to colonial rule and exploitation, through localized and sporadic withdrawals, maroonage, banditry, raids, work slow-downs, destruction of European property, and then rebellions. According to Isaacman, the rebellions indicated a new level of consciousness because Africans were forming multi-ethnic alliances and conducting guerrilla warfare to regain independence.

As Isaacman himself states, the book does not discuss the sociological and institutional dynamics of African societies, especially the secondary states. Furthermore, it seemingly neglects the ongoing realignments and rehabilitation among the African societies themselves. The resultant dichotomous relations of new aristocracies and peasants, alien and indigenous, haves and have-nots, within and among African societies, were also determinants of African responses. I wonder too if some of the succession crises, such as the Barwe crisis (1887-1901) between the moderate, Westernized, pro-Portuguese Samacande and the militant, traditionalist, anti-Portuguese Hanga, which Isaacman treated as weaknesses inherent in African societies, could not be treated as ideological struggles.

In all, the book is certainly provocative, interesting, well written, and extensively documented with primary sources and oral traditions. It significantly contributes to the study of African responses to colonialism in Mozambique in particular and Africa in general, and provides stimulation and leads for further research.

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Northridge

MAHMOOD MAMDANI. *Politics and Class Formation in Uganda*. New York: Monthly Review Press. 1976. Pp. viii, 339. \$16.50.

Three important books on post-independence politics in Uganda have been published in the last year, each with a distinctive theoretical focus. Nelson Kasfir (*The Shrinking Political Arena*) examines Ugandan politics through the prism of ethnicity, while James Mittelman (*Ideology and Politics in Uganda*) places ideology at the center. Mahmood Mamdani applies a Marxist framework in an interesting test of the uses and limits of Marxist methodology in understanding political change in contemporary Africa. In my opinion it is the weakest of the three.

The Mamdani study was initially a Harvard dissertation; the author is an Indian who grew up in East Africa. He thus brings to his research the intuition of events which comes from personal im-

mersion, joined to careful documentary inquiry, albeit concentrated on the economic sphere. His purpose is to provide a Marxist explanation of politics under Obote and Amin from 1962 to 1972; however, two-thirds of the text is devoted to the colonial period, as Mamdani seeks to demonstrate the class formation process.

He argues that both the Obote and Amin regimes are to be understood as the instruments of an insecure petite bourgeoisie, itself the dependent appendage of international capitalist imperialism. The petite bourgeoisie is composed of traders, bureaucrats, and "kulaks," a rather poetic term for cash crop farmers who till three-to-five acre farms and hire occasional or even regular labor. This group, at the time of independence, was divided into Buganda (the heart of kulakdom) and non-Buganda segments; in the early years of independence the Buganda kulaks were brought to heel by the central petite bourgeoisie. They also nipped in the bud an embryonic working-class movement, by domesticating and co-opting trade-union structures, and expelling Kenyan workers in 1970. The bureaucratic segment of the petite bourgeoisie, which the Obote regime represented, lost control of its armed forces, which with the support of the trader-kulak petite bourgeoisie assumed power by coup in 1971. There followed, then, a decisive confrontation with the Asian-dominated commercial bourgeoisie, leading to the expulsion of the latter in 1972.

While the Mamdani application of Marxist analysis yields some intriguing insights, it is also frequently quite mystifying. The application of the Marxist concept of feudalism to precolonial Buganda is quite unsatisfactory; indeed, the author is not consistent in dealing with the *bakungu* chiefs, who are variously feudalists becoming a mercantile class (pp. 28-29), an entrenched feudal caste (p. 128), and a compradore bourgeoisie (p. 174). The critical importance of ethnicity and religious conflict (the latter greatly underplayed by Mamdani) cannot be reduced to class labels. It is astonishing to learn that the capricious terrorism of Amin is simply an artifact of imperialism (p. 316), when Western interests have long abandoned the country. Rather than the instrument of liberation which the author intends, in this study the Marxist method seems more like an analytical prison.

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ASIA AND THE EAST

DAVID KRANZLER. *Japanese, Nazis and Jews: The Jewish Refugee Community of Shanghai, 1938-1945*. Foreword by ABRAHAM G. DUKER. New York: Yeshiva University Press; distributed by Sifria Distrib-

utors, Brooklyn. 1976. Pp. 644. Cloth \$17.50, paper \$7.95.

This is a painstaking documentation of a vignette in Jewish history, a collage of information about 17,000 Jews who found refuge from the German Holocaust in the remote but open International Settlement in Shanghai, China, at a time when doors to every other country were shut to them. David Kranzler collects every possible source of information about the refugees' plight, having contacted survivors on every continent for information on their displacement from native lands and drawing together the political, economic, social, and other threads tying them to Shanghai.

Here, upon arrival, they encountered two established Jewish communities, each with comfortable and sometimes luxurious niches carved out in a city known for its opportunistic—and at times amoral—international population. As the numbers of refugees swelled between 1938–40, conflicts between the local Jews and the newcomers increased daily. Both the Sephardic and the Russian Jewish communities extended themselves to accommodate the German, Austrian, Polish, and Lithuanian refugees. They too, however, had to survive in the hostile and oppressive environment.

Kranzler has access to fascinating information about the peculiar Japanese interpretation of anti-Semitism. According to him, few Japanese had any knowledge of Jews or Judaism before the mid-thirties, when Jewish populations suddenly became part of Japanese domain in the occupation of Manchuria and North China.

According to Kranzler, a few middle-echelon officers were first exposed to anti-Semitism and theories about international Jewish plots to take over the world by White Russian officers during the Siberian campaign in 1918–22. It was the same officers who now found themselves formulating Japan's policy toward the Jews. This policy reflects an interesting duality: the crude concepts of anti-Semitism on the one hand, and a genuine belief in the power and influence of Jewish wealth within government circles in the United States, Great Britain, and France on the other. Thus, the Japanese in China were apparently not interested in eliminating the allegedly powerful and wealthy Jews, but rather were determined to captivate their power and utilize it for Japan's own Greater East Asia Co-Prosperity Policy.

Kranzler's description of Shanghai, of life in the Heime (refugee camps) and the International Settlement in general, the resourcefulness of local refugee relief committees, and even more so, of the refugees themselves, is a saga patched by hundreds of footnotes and references which whet the reader's appetite for longer narratives. The survival of the harassed community, threatened, remote, and isolated, is revealed in glimpses through Kranzler's

massive documentation, signalling possibilities for further incursions into this episode in Jewish history.

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CHAE-JIN LEE. *Japan Faces China: Political and Economic Relations in the Postwar Era*. Baltimore: Johns Hopkins University Press. 1976. Pp. xiii, 242. \$12.50.

Outside Japan, the degree to which relations with China have profoundly affected domestic Japanese politics is seldom realized. Traditional cultural ties, the impact of the great Pacific war, and, in the postwar era, the symbols of cold war ideological conflict have in turn been reflected through the prism of domestic politics on Sino-Japanese relations. One of the arguments of this succinct study is that more pragmatic considerations—economic transactions, strategic elements, and Northeast Asian regional affairs—are more likely to characterize future relations between the two countries.

The purpose of the volume is three-fold: to describe the broad pattern of Sino-Japanese political and economic relations, 1949–72; to discuss the "politics of linkages," especially among the majority Liberal-Democratic Party (LDP), the opposition led by the Japan Socialist Party (JSP), and the two Chinas; and to assess the effects of inter-party competition upon unfolding relations with China.

The author has paid special attention to the means by which the ruling LDP and the opposition JSP have manipulated domestic and foreign affairs. In doing so he has examined the global and regional environments, the articulation between parties and business, trading companies, and labor federations, pluralistic expressions of public opinion, electoral results, parliamentary maneuvers, and bureaucratic cleavages. Looking always at the one issue, the book nonetheless offers further insight into critical factional differences, varied leadership styles, and the complex decision-making process.

Chae-jin Lee readily admits that his work was constrained by the "asymmetry of accessible sources" as between Japan and China. His facility with the necessary Asian languages, however, enabled him to utilize indigenous sources. He also handled quantitative data from trade statistics, opinion polls, election results, and voting records. In addition, he has interviewed numerous Japanese scholars, students, businessmen, and politicians, as well as members of the Chinese Embassy in Tokyo.

On occasion, the style of the book is marred by a seeming compulsion to force data into current behavioral molds. For example, the author refers to

the LDP and a "reactive process—one of three linkage processes identified by Rosenau," and states that such a process "was certainly conceivable in the LDP's China policy" (p. 12).

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PING-TI HO. *The Cradle of the East: An Inquiry into the Indigenous Origins of Techniques and Ideas of Neolithic and Early Historic China, 5000–1000 B.C.* Chicago: University of Chicago Press. 1976. Pp. xxi, 440. \$20.00.

There was a time in the not too distant past when, on the basis of a modicum of evidence and more than a bit of Western cultural chauvinism, it was claimed that civilization originated in the Fertile Crescent and spread from there to all corners of Eurasia and, perhaps, even to all parts of the earth.

More evidence is now available. In *The Cradle of the East*, Ping-ti Ho, one of the foremost students of his country's history, has marshaled these new findings brilliantly to demonstrate the independent origins of the characteristic features of Chinese culture. He discusses the indigenous evolution of the domesticated plants and animals basic to Chinese agriculture, the features of the loess soil region of North China (which caused its early neolithic inhabitants to live in large, permanent settlements where field agriculture greatly overshadowed the marginal returns from animal husbandry), and the unique technology and style of pottery and bronze wares. The author also establishes the neolithic origins and autochthonous development of Chinese numbers, written words and language, of ancestral worship, divination procedures, and fundamental forms of social and political organization.

Here are the dry bones of archeology and textual analysis turned into a vital reconstruction of the past. Pollen grains help establish the neolithic climate of the loess area. Kiln types and mineral deposits tell of metallurgical capabilities, and pottery markings reveal early numerals. Documentary evidence is added to material finds to show the comparatively late use of beeswax and thus of *cire-perdue* casting, and the introduction of irrigation techniques only well into historic times.

Some cultural borrowings from the outside appear and are investigated. Ho shows all to be of comparatively late date, coming after basic patterns of Chinese life had been fixed. Overall, his wide-ranging integration of materials from many sources presents a clear picture of Chinese chronological development, centers of cultural growth, and the gradual emergence of contacts with peoples on the periphery of the Chinese world.

The book is not without faults. Little attention has been paid to the work of recent Japanese scholars. Even more seriously, some evidence, particularly that drawn from traditional texts, seems to have been forced into the general mold of Ho's conclusions. To me, the argument for humanism and rationalism in early Chou times, based on evidence from late Chou and Han sources written when Chinese scholars were, indeed, humanistic and rationalistic, is especially weak.

On the whole, however, he makes comprehensive and convincing case for China's cultural autonomy. In his words, "Chinese civilization was just as pristine as the Mesopotamian and in terms of originality could claim equal primacy" (p. 368).

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HERRLEE G. CREEL. *Shen Pu-hai: A Chinese Political Philosopher of the Fourth Century B.C.* Chicago: University of Chicago Press. 1975. Pp. ix, 446. \$14.50.

To the present reviewer, the most fascinating and heuristically significant aspect of Herrlee G. Creel's masterful study of Shen Pu-hai (d. 337 B.C.), the innovative administrative philosopher of ancient China, is not only his unusual interpretive method but also the scholarly procedure by which his claims are articulated. It is not very informative simply to note that the dean of Sinology has once more made quite a few highly controversial observations, even though some of them are truly exceptional. One of the main theses of the book, that Shen Pu-hai's political thought is a kind of administrative philosophy which was neither influenced by Taoism nor swayed by Legalism, is itself a candidate for critical re-examination.

Creel's painstaking effort at the reconstruction of Shen's thought by an exhaustive research on the Shen Pu-hai fragments again demonstrates that Creel is not, in his own words, a "normal" Sinologist. The extraordinary quality of the book is best shown in Appendix C where twenty-seven fragments are translated with detailed annotations. Although some readings, notably 4 and 20, seem debatable, the overall performance is definitely a *tour de force*. It is not far-fetched to suggest that each word appears to have been carefully weighed to determine its value in an integrated structure of meaning against which the relative importance of each fragment is assigned. Even in the debatable readings, other interpretive possibilities are noted and the author's choices explained. Creel's independent-mindedness and conscientiousness are also manifested in his brief comment on the reconstructions of the Shen-tzu (Appendix B) and in his lengthy justification for the authenticity of the Shen Pu-hai fragments (Appendix A).

Methodologically, the subject matter is telescoped into a universe of its own, the surrounding world transformed into background whose varying degrees of relevance depend upon the angles and distances the observer chooses for his focused investigations. Thus, Shen Pu-hai, to whom the authoritative *Historical Records* (*Shih-chi*) devotes a total of sixty-eight Chinese characters, is strategically enlarged to the stature of the most original administrative thinker in pre-Ch'in China. The shape of Shen's political thought, Creel argues, was mainly determined by Shen's attempt to "devise a method by which the ruler might maintain control on the basis of nothing more than administrative technique and applied psychology." Creel further asserts that a close analysis of the available material indicates that Shen was probably the originator of the following administrative insights: (1) the principal emphasis for government should be placed upon rational organization and technique in a spirit of moral detachment, (2) the important function of the ruler is merely to preside over his ministers, not to do anything, (3) since no large-scale administration is possible without categorization, a system of names describing administrative methods are necessary, and (4) the use of tallies as a means of authentication should be implemented as an integral part of the technique to bestow office according to the capacity of the candidate and to demand actual performance in accordance with the title.

To convince us that these salient features of Shen's thought really have far-reaching theoretical and practical implications, Creel interlaces his interpretation with supporting quotations from modern Western authorities on bureaucratic and administrative theories, such as Peter Blau, Lyman Bryson, S. N. Eisenstadt, Robert Merton, and Herbert Simon. He also cites references from Machiavelli, Francesco Guicciardini, and Max Weber to show that Shen did address himself to some of the perennial concerns in the art of large-scale government. An implicit consequence of this trans-temporal and cross-cultural analysis is the demand for a more refined understanding of Fa-chia (commonly and, according to Creel, wrongly rendered as the Legalist School). If Creel's interpretive position can be maintained, Fa-chia will have to be differentiated into the Legalism of Lord Shang and the antilegalist administrative philosophy of Shen Pu-hai. As a result, Shen Tao, another important architect of Fa-chia in traditional scholarship will be relegated to the background, and Han Fei will be characterized as a somewhat muddleheaded synthesizer of the Legalist wing of Fa-chia.

Questions of perspective arise from Creel's attempt to establish the significance of Shen Pu-hai. To be sure, Creel admits that "to measure the

influence of the thought of Shen Pu-hai before the Han dynasty began, in 206 B.C., is difficult; after that time it becomes impossible." But almost one hundred pages are devoted to a comprehensive search for Shen's influence on traditional Chinese political theory and practice. From the perspective of historical significance it seems that Creel's *Problematik* mainly lies in his concerted effort to make Shen's administrative philosophy pure. This quest for purity appears to have seriously restricted the symbolic resources which Shen may have actually tapped to formulate his realistic vision of the principles of government. The newly discovered Fa-chia texts, *Ching-fa*, *Shih-la ching*, *Ch'eng*, and *Tao-yuan* may lead to revaluation of Shen's historical moment as well as his intellectual message.

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MICHAEL LOEWE. *Crisis and Conflict in Han China, 104 BC to AD 9*. London: George Allen and Unwin; distributed by Rowman and Littlefield, Totowa, N.J. 1975. Pp. 340. \$20.00.

This book is a collection of nine essays dealing with various aspects of Han China during the first century B.C. From a fresh point of view, the author discerns in the historical developments of the Former Han period two basic attitudes which he labels, respectively, as the Modernist and the Reformist. The Modernists promoted policies with a view mainly to strengthening the power and wealth of the state as well as the authority of the throne, whereas the Reformists were concerned basically with the lessening of hardships in people's lives. The Modernists are so called because they derived their tradition from the immediate past of the Legalist Ch'in empire. On the other hand, the Reformists took as their model the idealized remote past of the "benevolent government" of the Chou dynasty. Nowhere is the contrast between these two attitudes more clearly shown than in the "Grand Inquest" of 81 B.C., a debate on government monopolies of salt and iron (pp. 91-112). In this debate, the Modernists stood firmly for state control of these major industries while the Reformists of Confucian persuasion criticized the government for competing with the people for profit.

In terms of schools of political thought in Han China, these two contrasting attitudes correspond very closely to what have been traditionally called Confucianism and Legalism. But the author's new characterizations are more descriptive of the realities of the time than the traditional labels. For, as a result of the Han syncretism in ideas, neither Confucianism nor Legalism could be found in its pure form. In this connection it may be pointed out that

Michael Loewe's analysis of the two conflicting ideologies also has the additional merit of being current; it helps scholars evaluate the discussions on the so-called "struggles between the Confucian line and the Legalist line" in Han times by historians and propagandists in China today.

The book follows a strict chronological order. It begins with the first year of the reign period known as Grand Beginning in 104 B.C. But, ironically, the Grand Beginning turned out to be the onset of decline of the Han power; it was a time when the Modernist attitude was about to give way to the Reformist attitude. The intensification of conflict between Modernism and Reformism led to a number of political incidents and crises, which Loewe treats in considerable detail, notably "The Case of Witchcraft in 91 B.C.," "The Fall of the House of Huo—68–66 B.C.," "K'uang Heng and the Reform of Religious Practices," and "The Punishment of Chih Chih—36 B.C." The book concludes with the culmination of conflict in the enthronement of Wang Mang in A.D. 9, which brought the Former Han dynasty to a close. All these significant events in Han history are here presented to the Western reader with clarity.

YING-SHIH YÜ
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CHI-YUN CHEN. *Hsün Yüeh (A.D. 148–209): The Life and Reflections of an Early Medieval Confucian*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press, 1975. Pp. x, 242. \$22.50.

In the present state of Western scholarship on Chinese history, the Age of Disunity, between the end of Han and the Sui-T'ang reunification (roughly 200–600), is least adequately covered. With the exception of a few monographic studies, mostly annotated translations of historical texts, the whole period of early medieval China remains largely a *terra incognita*. The publication of Chi-yun Chen's *Hsün Yüeh* is therefore doubly welcome.

This book is a study of the life and writings of Hsün Yüeh, based originally on the author's doctoral work done at Harvard a decade ago. It consists of seven chapters—two on the political, social, and intellectual conditions of the time in which Hsün Yüeh lived and wrote, one on Hsün's family background and bureaucratic career, and two on his works. As the author's learned discussions in the footnotes show, the entire edifice of the study is built on a firm textual ground with solid philological bricks.

Though practically unknown to the non-specialist reader in the West, Hsün Yüeh occupies a prominent place in the Chinese intellectual tradition both as historian and as thinker. In the history

of historical writing, Hsün Yüeh's *Han-chi* (Chronicles of Han) served as an important link between the *Tso Chuan* (Tso Tradition) of the pre-Ch'in period and the *Tzu-chih t'ung-chien* (Comprehensive Mirror for Aid in Government) by Ssu-ma Kuang (1016–86), the greatest masterpiece in the annalistic tradition of Chinese historiography. However, since the rise of Ch'ing philology in the late seventeenth century, the *Han-chi* has been valued by scholars mainly because it occasionally differs from the present text of the *Han-shu* (History of the Former Han Dynasty), of which it was originally a condensation. Through careful analysis, the author has succeeded in rehabilitating Hsün Yüeh as an original and creative historian. It is now clear that beyond the value of textual collation the *Han-chi* also has its own intrinsic historiographical merits.

On the other hand, Hsün Yüeh the thinker is less distinguished than Hsün Yüeh the historian. His ideas as expressed in the *Shen-chien* ("Extended Reflections") are not particularly original. As the author rightly points out, "the *Shen-chien* is an assemblage of old sayings, but addressed to a new audience" (p. 136). In the history of thought in the Han period, Hsün Yüeh not only pales beside Tung Chung-shu (179?–104? B.C.) and Wang Ch'ung (A.D. 27–ca. 100), but even ranks next to Wang Fu (early second century) and Chung-ch'ang T'ung (179–220). But the very fact that Hsün Yüeh is a minor thinker makes him a better subject for the study of intellectual trends of his own times. For, as G. H. Palmer once said, "The tendencies of an age appear more distinctly in its writers of inferior rank than in those of commanding genius."

The period in which Hsün Yüeh lived witnessed a fundamental intellectual transformation from political criticisms of the literati (known as *ch'ing-i* or "pure criticism") to a movement of philosophical conversations transcending all mundane matters (known as *ch'ing-t'an* or "pure conversation"). Hsün Yüeh's works reflect this transformation vividly. As the author has aptly summed up, "The *Han-chi* was the last major product of the *ch'ing-i* positive political endeavor in the later Han; the *Shen-chien* marked the beginning of the *ch'ing-t'an* evasion and withdrawal from active involvement in the Age of Disunity" (p. 162).

To conclude, however, I find it necessary to make a few remarks on the transition from the *ch'ing-i* to the *ch'ing-t'an*. First, throughout the book the author makes references to both movements, but gives no systematic account of the transition. For the benefit of the non-specialist, such an account seems highly desirable. Second, when the author occasionally touches upon the problem of the transition, he tends to understand it mainly in

terms of the disintegration of sociopolitical order at the end of Han. To be sure, the validity of this long-held view can hardly be questioned. Nevertheless, evidence strongly suggests that the rise of the *ch'ing-t'an* movement must also be interpreted as an internal intellectual development since the end of the second century. Third, the term *ch'ing-t'an* has two different shades of meaning. In its earlier usage during Hsün Yüeh's time, it was often interchangeable with *ch'ing-i* in meaning. *Ch'ing-t'an* did not acquire its transmundane philosophical meaning until the middle of the third century. Failure to make this important distinction has somewhat affected the author's understanding and translation of two interesting letters exchanged between Chung Yu and Ts'ao P'i in 219 (p. 146).

Criticism aside, this book breaks new ground and contributes substantially to our knowledge of early medieval Chinese history.

YING-SHIH YÜ
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MARGERY WOLF and ROXANE WITKE, editors. *Women in Chinese Society*. Stanford: Stanford University Press, 1975. Pp. x, 315. \$12.50.

This book contains ten essays presented at a 1973 conference on Chinese women. Multidisciplinary in nature, it intends to provide a basis for understanding the massive change in Chinese women's lives in our time.

The time span covered starts as early as the sixteenth century. Joanna Handlin in her "Lu K'un's New Audience" assumes "widespread women literacy" in the Ming dynasty, which is not convincing, since a few individual cases do not prove the extent of literacy in relation to the total female population. Neither the participation of women in rebellions nor the phenomenon of henpecked husbands is peculiar to the Ming; they simply do not indicate the "growing power of women" at that time. In dealing with the versatile revolutionary Ch'iu Chin, Mary Rankin in her "Emergence of Women at the End of Ch'ing" regards the heroine as representing the general trend toward liberation since late Ming. But Ch'iu's life story shows only the peculiarities of her case; no more than one percent of Chinese women were permitted to study, ride horses, and use swords as she did. Instead of accepting her unhappy marriage as her fate, Ch'iu left her husband and joined the revolution. Hers was an exceptional case and should probably be dealt with as such. Yi-tsi Feuerwerker is right when she writes in "Women as Writers in the 1920s and 1930s" that the aspiring women writers in China suffered not only from the absence of a favorable tradition, but

also from the presence of a highly unfavorable one. It was more so at the turn of the century when Ch'iu Chin lived. Emily Ahern's "Power and Pollution of Chinese Women" shows that even today women are regarded as ritually unclean and dangerously powerful, and they are barred from certain activities. Roxane Witke's biographical essay on the early years of Chiang Ch'ing conveys the pains of breaking away from social conventions.

Margery Wolf in "Women and Suicide in China" makes a great contribution by her finding that Chinese data refute Western research that maintains men are more likely to commit suicide. The pattern of women's lives varied widely from one area to another and from one period of history to another within the same area. Both Marjorie Topley's "Marriage Resistance in Rural Kwangtung" and Elizabeth Johnson's "Women and Childbearing in Kwan Mun Hau Village" deal with women in specific areas at different periods. Arthur Wolf in his essay "The Women of Hailan" emphasizes that the phrase "women in China" should appear in quotation marks until we discover the core of experience that justifies the use of the term.

This collection is a pioneering scholarly effort in exploring the role of women in Chinese society. The work of historians and that of anthropologists complement each other. Despite a few controversial points, it is commendable reading for students of modern China.

CHIA-LIN PAO TAO
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DONALD A. JORDAN. *The Northern Expedition: China's National Revolution of 1926-1928*. Honolulu: University Press of Hawaii, 1976. Pp. xvi, 341. \$14.00.

This book provides a detailed chronological description of the Northern Expedition, the 1926-28 military campaign to eliminate warlord power and unite China. The first of its five parts details political activities during 1925-26 in the revolutionary base at Canton. It is followed by an account of the military events of the Northern Expedition. Part three deals with the role of the masses during the campaign, and part four examines the actions of the political arm of the Northern Expeditionary Armies. The final section describes how warlords were enticed to defect to the side of the revolutionaries.

Donald A. Jordan is particularly interested in refuting the notion that the Chinese masses made a substantial contribution to the military achievements of the expedition. Harold Isaacs' passionate assertion that a tidal wave of popular support swept the armies to the Yangtze originally sparked Jordan's interest in the subject, but in this work he

is largely concerned with repudiating Chinese Communist publications.

Jordan is surely correct to argue that military victories cleared the way for expansion of the mass movement rather than the converse, and his discussion provides much detail about the precise chronology and character of mass organization and its relation to the advance of the armies. The section on the army's political department is also interesting and useful; it draws heavily on personal interviews with various individuals on Taiwan, and stresses the positive response of the peasants to the army's generous and fair treatment of civilians.

Chinese history of the mid-twenties embraces many still-controversial events and individuals, and Jordan's treatment will not end the disputes. In some instances, he simply accepts one side without discussion, as in his description of the "Green Society" as a "traditional secret society of local workers" (p. 124); this description will appear absurdly euphemistic to those who conceive of the "Green Gang" as a powerful underworld organization. In other cases, Jordan adduces evidence that genuinely challenges leftist interpretations, but uses it in a slightly ambiguous fashion, as in his discussion of the taking of Shanghai.

The comprehensive detail, which is one of the book's assets, does not make for easy reading, and the writing is rather strained. Moreover, Jordan's concern to refute Communist writings gives his work a polemical flavor, and tends to minimize other important phenomena. The peasant and the labor movements, for example, appear as if they had no meaning except as pawns in a struggle between the Kuomintang and the Communist Party. The Communists emerge from Jordan's pages as completely preoccupied with maneuvers for party power, not as real people who were committed to particular conceptions of what China desperately needed.

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G. CAMERON HURST III. *Insei: Abdicated Sovereigns in the Politics of Late Heian Japan, 1086-1185*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1976. Pp. xii, 337. \$17.50.

Toward the end of the Heian period (794-1185) the Japanese imperial house shook off the dominance of powerful Fujiwara regents and for a century held political leadership in the court. The symbol and mechanism of this re-emergence into power was the *Insei*, the offices of retired emperors. From 1086 to 1185 the imperial family under the lead-

ership of strong emperors and retired emperors extended its influence over the court, expanded its house offices, and acquired huge amounts of private lands.

G. Cameron Hurst blends several styles in his explication of these events. Japanese political and cultural concepts required careful explanation; the political history of late Heian Japan wanted precise narrative. The resultant mixture makes the book slow reading, but the author is conscious of his style and is a careful writer.

Insei is a thorough examination of Japanese court politics. It will be especially valuable to those interested in political theory (Japanese concepts of "legitimacy" and "sovereignty"), clientage (patron-client relationships in premedieval Japan), cities (Kyoto, capital in the eleventh and twelfth centuries), land contract and ownership patterns (new materials on private lands acquired by the imperial house in the *Insei* period), family history (Heian court aristocracy and kinship groups, especially the re-emergent imperial house), documents (discussion of court diaries, state documents, private house materials, and official and private court histories), and Japanese historiography. The book offers a thoughtful revision of Heian political history.

THOMAS H. MESNER
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I. I. VASILEVSKAIA. *Kolonial'naia politika Iaponii v Koree nakanune anneksii (1904-1910 gg.)* [Japan's Colonial Policy in Korea on the Eve of Annexation, 1904-1910]. (Akademiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1975. Pp. 116. 64 k.

I. I. Vasilevskaia's monograph attempts "to show the various forms and methods of economic robbery . . . and also political enslavement of Korea . . . which signified the virtual transformation of Korea into a colony of Japan even before its annexation in 1910." Its three chapters cover Japan's occupation of Korea during the Russo-Japanese war and establishment of the protectorate in 1905, the measures by which Japan subordinated Korean political processes to the Resident-General's supervision, and the protectorate's economic effects in hindering the growth of Korean industry and impoverishing the Korean people. The author concludes that annexation was the inevitable outcome of Japan's imperialist policies, delayed during the five-year protectorate only while Japan suppressed the Korean opposition and obtained assurances that the other powers would not intervene.

As a narrative, Japan's subordination of Korean politics and economy is not difficult to demon-

strate, but in doing so the author breaks no new ground. The material of her second chapter is already adequately covered (and with greater attention to the political complexities involved) in Kim and Kim's *Korea and the Politics of Imperialism*, while most of the data for the third chapter come from Japan's official *Annual Report[s] on Reform and Progress in Korea*. Vasilevskaia relies mainly on English- and Russian-language sources, using some basic Japanese but virtually no Korean sources. The monograph's strength lies in its systematic presentation of the administrative and judicial changes Japan imposed on Korea and its descriptive listing of techniques used to infiltrate the Korean economy, but it does not add significantly to the store of information already available to the English-reading scholar.

The monograph's deeper difficulties lie in its failure to set the protectorate in the context of East Asian political history and in its concentration on the "how" (which we already know) without elucidating the "why" of Japan's aggression. The author's teleological view of the annexation, based on Marxist-Leninist assumptions about the expansionist nature of capitalism, leads her to describe the Japanese political scene in distortingly broad terms, glossing over, for example, distinctions between the gradualist policies of civil officials like Itō and the intense pressure for annexation from militarists like Uchida. Thus, the monograph's evident biases and tendency to interpret all issues in black-and-white terms further reduce its usefulness.

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RICHARD H. MITCHELL. *Thought Control in Prewar Japan*. Ithaca: Cornell University Press. 1976. Pp. 226. \$10.95.

The purpose of this slender volume is to "investigate" the legal institutions and procedures which prewar Japanese Justice Ministry officials utilized for thought control, and to do so in such a way as to "present official views fairly and objectively" (p. 13). The book focuses on the Peace Preservation Law of 1925 and subsequent refinements of this legislation by Justice Ministry officials, culminating during the 1930s in administrative extensions and reinterpretations of the law to include not only illegal left-wing organizations in Japan but virtually all forms of dissent against state policies. Considerable attention is devoted to examining how the laws were applied, and how the policy of seeking ideological conversion (*tenkō*) and rehabilitation of "thought criminals" distinguished the Japanese case from other thought-control systems. Richard H. Mitchell's main con-

tribution is his description of justice officials' thinking, and his illustrations of how they perceived challenges posed to existing norms of ideological acceptability. Readers will also be grateful for the introduction to the range of published and unpublished Justice Ministry documents currently available for scholarly use.

Despite the author's explicit commitment to objectivity, however, his study is characterized by a tendency to accept and defend Justice Ministry policy positions uncritically. Mitchell's narrative clearly supports the Ministry's view that the Peace Preservation Law was necessary (p. 67), although he offers little new evidence to support this heretofore questionable position other than the opinions voiced by Ministry officials. He passes over the issue of brutality in thought control with a narrowly defined discussion of coercive force, which leaves only low-ranking police personnel culpable (p. 101). He insists Ministry officials were no more "evil" than others who supported state policies, without critically evaluating the extent to which their "politicization" led them to violate the very norms of justice they were appointed to uphold. The dire consequences of restricting open political discussion are not treated; the "prosecutors' frameup" in the Teijin Incident—the most blatant intervention by Justice officials into the political process during the 1930s—is not even mentioned.

Mitchell's explanation of *why* the thought-control system developed is similarly puzzling. Notwithstanding his attack on historical determinism (p. 11), the author's elucidation of the growth of Justice Ministry power seems deterministic: the ease with which power could be abused and the extent of their power led *naturally* to officials' politicization (p. 36); under wartime conditions, the thought police *naturally* became more deeply involved in citizens' lives (p. 159); and once the state's control mechanism "had crushed the communists and radical socialists it *quite naturally* moved on to the others" (p. 182, *italics mine*). This "natural" progression does little to explain why the growth of Justice officials' power was permitted.

Indeed, little new light is cast on the causes or extent of Japanese anxiety over ideological heterodoxy, because the author relies heavily on interpretations from secondary sources for his causative explanations of these and other vital issues. This dependency is particularly regrettable in the numerous instances where the secondary source cited is one of several dissertations whose authors have not yet themselves had full opportunity to publish their views.

GORDON MARK BERGER
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JON HALLIDAY. *A Political History of Japanese Capitalism*. New York: Pantheon Books. 1975. Pp. xxxiii, 466. \$15.95.

This volume makes available an English-language Marxian analysis of the political economy of modern Japan. This is a valuable contribution. In contrast to the dominant role of Marxian analysis in Japan, the questions and issues raised by Marxian scholars are not easily accessible in English. However, two factors make the present analysis inconsistent with Japanese scholarship, namely the European-centered conceptual framework and the author's dependence on Western-language sources. The first leads to some rather unusual interpretations, and the second both limits the data base—although Jon Halliday makes good use of the available bibliography—and denies him a thorough exposure to his Japanese counterparts.

The analysis begins with a discussion of the Meiji state. Particular emphasis is given to the Meiji oligarchs' decision to model Japan on the most "advanced and repressive examples available." Here and elsewhere, Halliday offers moral judgments which do not always fit the Japanese case. Chapter 2 is a brief discussion of the "Beginnings of the Left," in which he sees the suppression of the labor movement and radical parties in the prewar period as the work of Japanese capitalism, rather than fascism.

Chapters 3 and 4 deal with Japanese imperialism and politics to the Washington Conference of 1922. The "capital poor" imperialism used by Japan to expand her influence in Asia and guarantee resources for industrial development constitutes a major theme. Chapters 5 and 6 take us from 1922 through the Pacific war. Halliday views the political regime as not undergoing a major shift from the 1920s to the 1930s because of fundamental agreement between the military and business on imperialist policy. For the Pacific war he gives emphasis to Japanese military brilliance and the support received from occupied colonial nations. Chapter 7 treats the "American Occupation" and the efforts by SCAP to preserve the conservative character of the ruling class in Japan. Japanese efforts to direct the course of occupation policy are discussed in a useful corrective to the conventional American-centered analysis. Here and in chapter 8, "Proletariat versus Capital Since 1945," Halliday notes that SCAP offered only "limited and conditional bourgeois democratic rights" and did not break the power of the Right.

Chapters 9 and 10 deal with "Education and the Student Movement" and "The Politics of Japanese Capitalism." Education is seen as repressive and geared to the interests of the state. He views "Tennoism" as serving the interests of capitalism and denying the existence of classes in Japan, and

Japan as tied to U.S. imperialism in Asia and as playing a supportive role behind the scenes since the Korean conflict.

Overall, this is an often provocative and interesting political analysis of Japanese capitalism. It is too dogmatic to be read uncritically, but it raises important questions deserving further discussion in and out of the classroom.

WILLIAM B. HAUSER
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UNITED STATES

HENRY F. MAY. *The Enlightenment in America*. New York: Oxford University Press. 1976. Pp. xix, 419. \$15.00.

Historians are a strange breed of scholars who make progress by moving backwards. Henry May exemplifies this conundrum in *The Enlightenment in America*. Having begun his career working on a later period, he has now moved back to the eighteenth century in order to understand better the American ideological compromise between a belief in moral certainties and a belief in the desirability of change and progress. In this new study he has searched for the roots of this compromise in the rich soil of the long eighteenth century which began with Locke's *Second Treatise* and ended with Waterloo. What May projects is the Enlightenment faith in liberalism, progress, and rationality grafted on to the basic American stock of Calvinistic Protestantism in four sequential cuts which he has labeled the Moderate Enlightenment, the Skeptical Enlightenment, the Revolutionary Enlightenment, and the Didactic Enlightenment.

The approach is taxonomic. The first three sections open with chapters describing the European themes to be transmitted, e.g. the Moderate Enlightenment introduces the Anglican admirers of Newton and Locke extolling natural religion; the Skeptical Enlightenment shifts from the London of Tillotson, Clarke, and Pope to the Paris of Voltaire and Diderot who upset the delicate balance of calvary and cosmic laws. The thematic introductions are followed by chapters detailing the American reception of the various Enlightenment ideas. Only the short last section on the Didactic Enlightenment breaks with this pattern, for here the story is not one of systematic reception, but of defeat and assimilation.

Despite the book's emphasis upon identification and arrangement, there is a story line, and that is that the Enlightenment grafts did not take. The Moderate Enlightenment informed America's Whig leaders who did not really need the Enlight-

enment since they drew principally upon classical republican ideas which seventeenth-century Englishmen had borrowed from Renaissance writers. The Skeptical Enlightenment—in May's view the least influential—created northern doubts and deists and a stoical South. The Revolutionary Enlightenment obviously had a better chance. The seminal ideas are English: Price, Priestley, Burgh, Paine, and Godwin. But even in this section where one would expect to find the roots of American ideology, the reception is mixed. New England clergymen feel beleaguered by a host of enemies, and the Connecticut Wits display a gentlemanly dislike of extremism. Things fare better for the Enlightenment in Philadelphia, but the last two chapters of this section return to the theme of rejection with examination of the High Federalists' counterattack and the abiding conservatism of Adams and Jefferson. In the final section, the Enlightenment disappears altogether, washed away in clerical circles by the tide of evangelical Protestantism and absorbed into Scottish Common Sense Philosophy among the upper-class intellectuals of the early nineteenth century.

May notes at the outset that scholarship on the American Enlightenment has been distorted by a preoccupation with politics. He does not repeat this error. Indeed, he is at his best when discussing the tensions between religious and revolutionary idealism. "The fact that American radicalism had long been nourished from Commonwealth rather than from French revolutionary sources," he explains, "made one major difference: in America a larger element of apocalyptic fear was linked with the period's millennial hopes." His emphasis upon the expectations created by the American religious outlook enables him to draw attention repeatedly to the incompatibility between the secular grafts and the religious stock. The pervasive religious nervousness he imparts to his story, however, tends to unsettle his classifying lines. Thus it is hard to distinguish between his conservatives roused by "religious apathy, immorality, ignorance and luxury" (p. 263) and the millenarian radicals who stressed the "importance of republican virtue as against aristocratic luxury and vice" (p. 156). And despite his unwillingness to ignore the Christian paradigm through which Americans interpreted the Enlightenment, May retreats from exploring the conflict. Thus Jefferson's election on the eve of the "greatest evangelical outburst in American history" is treated as an ironic coincidence (p. 304), as earlier it had been suggested that the Great Awakening may have reflected a change of style analogous to the move from neoclassic to romantic taste (p. 43).

May is quite explicit about not offering an examination of causes: "It is not part of the purpose of this book to answer the fascinating question of

the origins of the revolution of the late eighteenth century, which were certainly social and political and even technological as well as psychological and ideological." But causation does not disappear from the book. Having eschewed an analysis of material circumstances, May presents ideas in reaction to other ideas: "perhaps the only explanation of the change of mood is . . . that periods of consensus and compromise produce periods of intense controversy, and vice versa, in a cycle that reflects the basic ambivalence of human wishes." Similar allusions to the cyclical appearance of moods and to changes of style or temperament can be found on pages 43, 133, 155, and 179. These bring me to my reservation about the book: can ideas be treated coherently in isolation from the complex reality in which people use them?

Clifford Geertz has written that the sociology of knowledge ought to be called the sociology of meaning, for what is socially determined is not the nature of conception but the vehicles of conception. In the case at hand, the vehicles are words, but the meaning of these words can only be fully comprehended by learning what use people put to them. If such an analysis is not offered, the author unavoidably implies that we can take words at face value, that they do not require translation or interpretation in terms of the social context which gave them meaning. This is not to say that May has presented disembodied ideas. They are always identified with a person—355 different men and women, in fact! But his subjects hold ideas; they rarely create with them, feel constrained by them, fight with them.

This treatment of ideas as inert objects which can be catalogued in their human receptors contributes to the impression that the intellectual traditions described are pretty much irrelevant to what is really going on in America at the time. This assessment is also May's. Having started with the assertion that an understanding of the Enlightenment is basic to an understanding of America, he ends by saying that "often the Enlightenment, as promulgated by intellectuals, has been rejected by the people, or by large sections of the people." Evidently the real action was taking place somewhere else, but since all sentient beings must think before they act, it would be nice to know which other ideas were finding favor with the people during the Enlightenment period. There is another possible conclusion—a polemical one—which is that the reluctance to deal with ideas as elements in a socially-defined reality may be one of the most enduring legacies of the Enlightenment in America, and one which is shared by intellectuals and the people.

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WILLIAM APPLEMAN WILLIAMS. *America Confronts a Revolutionary World: 1776-1976*. New York: William Morrow and Company. 1976. Pp. 224. \$8.95.

During the past two centuries, according to William Appleman Williams, the United States frequently betrayed the fundamental principal which justified its birth as a nation: the revolutionary right of peoples to determine their own destiny. The underlying explanation is the obsession with preserving "the American Present," which has led to efforts "to control history" to such an extent "that we have distorted our ideals and ceased to deal effectively—let alone creatively—with reality" (p. 20). With the adoption of the Constitution, Williams contends, the founding fathers "killed Time (and History) in the name of uniqueness. They concluded that the Past was Bad and that the Future would very likely be Bad, and hence all that remained was the Here and Now" (p. 40).

Brief illustrations of this thesis in domestic affairs include the betrayal of self-determination for Indians, blacks, women, farmers, and laborers, who "were viewed by most as either hankering for the Bad Past or whoring after the Bad Future" (p. 127). The most provocative example, however, is reserved for the Civil War, the cause of which "was the refusal of Lincoln and other Northerners to honor the revolutionary right of self-determination" (p. 113).

Apart from whether America's leaders and citizens were practicing "counterrevolution to preserve the Present," a major thrust of Williams' argument is his criticism of the expansionist policies of the United States—territorial and economic. In this connection, few of America's political leaders escape censure. Thus Thomas Jefferson is faulted for having been "callous and condescending towards [the] French and Spanish inhabitants" following the acquisition of the Louisiana Territory. Such attitudes prove, in the author's judgment, that "America even then considered itself destined to self-determine the world." Harsh criticism is also directed at Woodrow Wilson, who "exercised all the great power at his disposal against American Socialists, intervened in counterrevolutionary ways in China, Mexico, and Russia and boldly proposed to recreate the world in the image of the American present" (p. 21). Among those Americans who seemed most committed to the ideal of self-determination, Herbert Hoover is praised for his opposition to global crusades, his rejection of Dollar Diplomacy, and his removal of American marines from Haiti and Nicaragua. Although Hoover presented "a limited vision of the Future" because "it accepted certain essentials of the capitalist ethos," it was one which "called upon all Americans to assert

their rights of self-determination to transcend the Present in a way that honored their principles—and their nature as human beings" (pp. 159, 160).

There is a great deal in Williams' analysis with which scholars would agree, particularly some of the ill effects of the sense of national uniqueness and destiny or the penchant for crusades. But there are instances when he oversimplifies the story, inadequately reflecting on the dilemmas which statesmen frequently encounter in trying to square idealistic principles with practical solutions to problems, or on the extent to which America's policymakers were reacting to events rather than manipulating them. Moreover, to suggest a virtually unbroken and conscious desire, from the founding of the Republic through the Vietnam debacle, to violate the principle of self-determination tends to impart more calculated rationality and consistency to America's decision-makers than the historical record warrants.

Those sections of Williams' study which should generate the liveliest debate deal with his principal recommendation that the United States return to a form of government embodied in the Articles of Confederation, which would entail "a federation of democratic Socialist communities." The author briefly discusses the entity of a Pacific Northwest Community (beginning with Washington, Oregon, Idaho, and Montana) with the Indian name *Neahkahnne*, "in honor of the Americans who first lived here." He anticipates criticisms of the regional communities he envisions, the possibility of violence in the process of reorganization, and a distinct weakening of the power of the United States to shape world affairs. Despite the prospect of any or all of these consequences, however, he is confident that the long-range results will benefit Americans in particular and mankind in general. "Once we begin to end the empire in America," he predicts, "we will exert pressure for similar reorganizations throughout the world." In view of recent centrifugal political forces that may be discerned in some parts of the world—e.g., the separatist movements of the French-Canadians in Quebec or the Basques in Spain—Williams' proposal has a certain timeliness about it which may find initial applications beyond America's borders. If so, our leaders might be able to learn some valuable lessons as to whether his proposal could indeed result in a Good Future for those Americans who would wish to determine their destinies by severing their ties with an American empire.

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FREDI CHIAPELLI *et al.*, editors. *First Images of America: The Impact of the New World on the Old*. In

two volumes. Berkeley and Los Angeles: University of California Press. 1976. Pp. xxii, 515; xviii, 519-957. \$75.00.

First Images of America is a very important collection of essays devoted to the early impact of the New World on the Old, and constitutes the record of an international conference held at UCLA. The essays range through art, philosophy, literature, history, and geography.

The Age of Discovery did not alter Europe's confidence in the appropriateness and superiority of its own institutions. The Portuguese returned with spices, not philosophy. European artists and travelers, drawing upon first-hand observations or secondary accounts, depicted the artifacts, dress, and customs of native Americans—the volumes handsomely reproduce a series of sketches, paintings, and Amerindian art treasures, which constituted the first visual images of America. But initial descriptions of indigenous societies evoked, at best, a fleeting interest that produced no enduring effect on Renaissance art and letters. The thoughtful reflections of Thomas More and Michel de Montaigne were exceptional; America did not transform Europe's image of itself. The recovery of classical antiquity rather than the discovery of pre-Columbian America reshaped Western culture. The artistic and architectural achievements of the Aztecs and Incas, for example, had scant impact in Europe—Indian artists learned to work in Spanish styles, not the reverse. Europe recreated itself in America.

Precisely for this reason, these volumes are tremendously important. In the widest sense, they raise questions about cultural exchange. Perhaps the disparities between the New World and the Old were too great. To European eyes, Amerindian America had the wrong gods, inferior technology, and abhorrent customs—nakedness, sexual promiscuity, and cannibalism. Secure in its own world-view buttressed by force of arms, Europe could not learn from the vanquished. Nor was this phenomenon confined to America. Europeans had trading colonies in Asia for centuries before Eastern art and philosophy seriously penetrated the West.

At first, however, the contact with America evoked curiosity and mutual forbearance rather than hostility. These volumes give us access to accounts that viewed America's cultural diversity sympathetically. Those who defended native cultures, upholding the humanity, rationality, and natural rights of the new peoples who justly inhabited America, filed a minority report essential to our own rediscovery of America's past. Bartolomé de Las Casas was not alone in questioning the justice of the Spanish conquest and the con-

duct of his fellow countrymen. The welfare of the Crown's Indian subjects was a sacred trust held in the name of Christ and subject to the principles of natural and divine law. America presented a moral and theological problem as real as its silver. Failure to appreciate this fact is to miss a basic aspect of America's impact on Spain.

The first images of America conveyed by Europeans frequently portrayed the generosity, constancy, egalitarianism, and civility of native Americans. Early Spanish priests like the great Franciscan ethnographer Bernadino de Sahagún devotedly detailed Aztec mythology and customs. North-American Indians attending the French court in 1562 expressed amazement at the "superior power held by unworthy persons," and noted that property was not held in common "there being in Europe some men full and gorged with all sorts of good things while others went emaciated with hunger and poverty." This exchange prompted Montaigne's famous essay, *Des Cannibales*. But the discovery of America did not produce an Indian critique of European society. Colonization gradually irradiated the pre-Columbian past. Sugar plantations in Portuguese Brazil destroyed the delicately balanced ecology that sustained indigenous social organization. Puritan towns and Chesapeake tobacco farms displaced the coastal tribes of North America. In Mexico, epidemics caused by European diseases devastated Aztec society, destroying essential forms of social organization that had perpetuated customs and traditions. The first images of America faded from view as America became the object of European rivalry and settlement. These volumes help us to understand the blunted impact of native cultures on the Old World, and they open a path for us to rediscover the rituals, artistic achievements, and human values of pre-Columbian America.

If the "clash of morality in the American forest" left few permanent traces in European thought, America provided a new arena for the extension and elaboration of European institutions. Once established, the new American economies reshaped European markets and commercial networks, creating new sources of wealth and taxation. Flows of sugar, tobacco, slaves, and silver circulated throughout the Atlantic world engendering a comprehensive price revolution, capital formation, and competition for empire. American colonization transformed European politics, enhancing the sources of power available to the state. Eventually, America even changed Europe's ecology as New World crops like potatoes, corn, and tomatoes became dietary staples. The Age of Discovery also began a far-reaching movement of people from Europe to America and back. Perhaps

750,000 Spaniards emigrated to the Indies between 1500 and 1700, and 300,000 Portuguese left for Brazil. In the years 1620-42, some 58,000 Englishmen went to continental North America and the Caribbean. The essays on Spanish migration are outstanding, presenting both a summary of new research, an excellent bibliography and an engaging introduction to the logic of historical demography.

In short, *First Images of America* presents a wide and challenging perspective on the character of the exchange between the New World and the Old. The essays are provocative, many of them superb and exciting.

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DWIGHT W. HOOVER. *The Red and the Black*. (The Rand McNally Series on the History of American Thought and Culture.) Chicago: Rand McNally College Publishing Company. 1976. Pp. ix, 469. \$4.95.

This is a useful study of the images of Indians and blacks in American thought. Its major source of strength is that it is concerned not merely with the matter of racial prejudice but with the more complex subject of how these ethnic minorities have been perceived, both by themselves and by white observers. Dwight Hoover is hard put for a good title for the book and finds himself obliged to explain in the preface that *The Red and the Black* has nothing to do with Stendhal's novel. The book covers the history of ideas of race since colonial beginnings in the seventeenth century. The best part is that which deals with the confused status both of racial theory and of attitudes toward racial minorities in modern times. In this area, it is probably the best book which has yet been written.

A source of weakness in the book is that insufficient attention is given to placing ideas of race in a meaningful context. The method is to move from book to book, from theory to theory. Only a specialist in the field is likely to be able to evaluate the worth of many of his sources. At one point, the author moves from a paragraph which paraphrases the ideas of Joseph Smith, the founder of the Mormon sect, to one which paraphrases the ideas of Samuel George Morton, a respected scientist of the nineteenth century, with no real attempt to explain the difference.

The weakest parts of the book are those which deal with the images of Indians and blacks in literature. The author has not sufficiently considered the question of whether a writer of fiction shares the opinions of his characters. Herman Melville has a character entitled the Indian Hater in *The Confidence Man*, but that does not warrant Hoover's conclusion that Melville "believed the Indian was depraved . . ." The character Jim in

Mark Twain's *The Adventures of Huckleberry Finn* is not a "Sambo," and certainly not because he is "superstitious and ignorant" or "trusting and loyal." Owen Wister was not a southerner, as Hoover seems to think (p. 102). Joe Christmas in Faulkner's *Light in August* is not a "mulatto." An important point in the book is that neither the reader nor Joe Christmas himself ever finds out whether the character has any black intermixture at all. If he does have any, it is very little since he can pass without question as a white man. It is misleading to say that Flannery O'Connor uses "the century-old stereotype of the Black as being Christlike." Most critics would deny that her characters are stereotypes and would also deny that their virtues or defects are necessarily connected with their race.

The book has other faults. It is carelessly proof-read, contains a good many words and expressions like "checkered past," "swept the field," "opt with the majority," "hung" for "hanged," "well-thought-out paradigm," and "different than." The author is a knowledgeable, intelligent, and perceptive man who has not yet learned his trade as a writer. On the other hand, I can think of no other book which introduces the reader to a greater range of the thinking concerning the supposed racial character of Indians and blacks.

THOMAS F. GOSSETT
Wake Forest University

JOSHUA C. TAYLOR. *America As Art*. Washington, D.C.: Smithsonian Institution Press, for the National Collection of Fine Arts. 1976. Pp. xi, 320. \$25.00.

DAVID C. DRISKELL. *Two Centuries of Black American Art*. New York: Alfred A. Knopf. 1976. Pp. 221. \$15.00.

The Bicentennial brought a stack of art books for the patriot's coffee table though the dates 1776 and 1976 have little significance for art history. Some books of substance were also timed for the jubilee.

America as Art is companion to an exhibition held at the National Collection of Fine Arts in Washington. Joshua C. Taylor is director of the National Collection, and he had to please no editor or publisher. The resulting book does not fit the usual categories. It does not cater to general readers; Taylor discusses few "big names" but many little-known artists; the illustrations are numerous but indifferently reproduced. It will not appeal to conventional art historians either; it is not a survey nor a monograph; the scholarly author provides no notes, bibliography, or index. He presents eight essays on episodes in American art, one of the eighteenth century, three of the nineteenth, and four of the twentieth. Some are on well-known

themes and others on unfamiliar topics, especially "The American Cousin" which deals with the canny rustics of ca. 1820–1860, exemplified by Uncle Sam or Asa Trenchard of *Our American Cousin*. In the hyper-specialized discipline of art history it is rare to meet a scholar like Taylor who writes with equal authority on eighteenth-century popular arts and on the chic Pop Art of the present. He is also deft in relating art to social and literary history. This book of original essays can be rewarding if the reader is already knowledgeable in art history.

Two Centuries of Black American Art is the elaborate catalogue of an exhibition at the Los Angeles County Museum of Art. It is divided into two parts, "Black Artists and Craftsmen in the Formative Years, 1750–1920" and "The Evolution of a Black Aesthetic, 1920–1950." The author has discovered no previously unknown geniuses, but he presents many artists of solid achievement. Two inevitable questions arise: "Is there a black character common to these American artists?" The candid author recognizes that black visual arts are generally in the mainstream of contemporaneous white art. Usually only black subject matter is a clue to the color of the artist. "Is there a visible African heritage in these American works?" Such influence survived only for a few generations in slavery. Some fascinating photographs of early Southern architecture and crafts display distinctive African forms. The book is well illustrated and carries a surprisingly massive bibliography. Driskell has produced a straightforward survey of marked initiative and interest.

JOHN MAASS
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THOMAS A. BAILEY, with the assistance of STEPHEN M. DOBBS. *Voices of America: The Nation's Story in Slogans, Sayings, and Songs*. New York: Free Press. 1976. Pp. viii, 520. \$12.95.

Thomas A. Bailey's contributions to historical literature have been noteworthy. His popularly written diplomatic history text was always a favorite, and this present work continues his tradition of writing history with verve and style. Bailey has now shown his versatility by entering the relatively new field of popular cultural history, an approach concerned with what people do with their leisure, what they eat, how they dress, etc. It is the history of everyday people in the pursuit of their daily lives, and as such is a sensitive barometer of attitudes, values, and mores.

Voices of America is a specialized treatment of American history as reflected in this nation's popular songs, slogans, and sayings. On the surface, it appears that it is just another superficial survey, but Bailey tells a familiar story by referring to

these subjects—some inspirational and some uplifting and others sheer, blunt political bromides. Some of what Bailey delineates is familiar, such as his discussion of such well-known phrases as "Give me liberty or give me death" and "Manifest Destiny." His close use of newspapers and periodicals has long been a Bailey trademark, while his stress on contemporary opinion makes his story timely. Some items may seem trivial and pleasantly nostalgic, but Bailey clearly demonstrates that they tell us much about our society, life-styles, and tastes. He correctly argues that such materials are the medium which best allows the masses to express their views and helps to focus on a common national purpose. He recognizes, however, that such an approach presents certain dangers.

Yet, for all of this volume's distinctiveness, certain references are missing which one would assume fits Bailey's framework. Graffiti, for example, are in effect slogans. From ancient times, graffiti dealt with sex, religion, and politics and, while most are and were gross and prejudiced, they too are a barometer of the everyday facts of life. Anyone traveling the New York subway network realizes the strength of the urge of otherwise inarticulate people to communicate. More emphasis could also have been placed on cartoons, advertisements, and the messages of campaign buttons, sometimes real but often humorous and certainly instructive.

While Bailey includes songs, he generally omits lyrics which also tell of the inner urgings of a people. For example, much of our knowledge of the West of the Gilded Age comes from such cowboy ballads as "The Old Chisholm Trail" or the ever-popular lament, "Git Along Little Dogies." Both are mentioned but not analyzed. The words to such selected songs as "Ta Ra Ra Boom Der E," "The Sidewalks of New York," "In the Good Old Summertime," "Barney Google," "Makin' Whoopee," "Brother, Can You Spare a Dime," or "I'll Never Smile Again" express eloquently the expansive mood of the Gay 90s, the yearning for the Good Years, the exuberance of the 1920s, and the anxieties of Depression and war. Rock music tells much about the youth protest of the 1960s, its drive for sexual liberation, the drug culture, and the revolt against an unpopular war.

Nevertheless, Bailey has demonstrated the importance of much of what has been treated as ephemeral. In his own colorful way, he has fleshed out history to a fuller extent, an effort which should prove especially valuable to history teachers concerned with holding the attention of apathetic students.

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JOHN DUFFY. *The Healers: The Rise of the Medical Establishment*. (Aspects of American Life and Culture.) New York: McGraw-Hill. 1976. Pp. ix, 385. \$12.50.

Prior to World War II medical history was written almost exclusively by doctors whose approach tended to be iatrogenic: physician-centered and professionally self-serving. Subsequently a small group of academic historians, led by the late Richard H. Shryock, sought to illuminate medicine's past by emphasizing the broader social and cultural dimensions of health, disease, and medical practice. Since the publication of *Epidemics in Colonial America* (1953), John Duffy, who is Priscilla Alden Burke Professor of History at the University of Maryland, has come to be recognized as one of the foremost historians of medicine in the United States. In the present volume Duffy has drawn upon his own extensive research, the work of Shryock, Whitfield J. Bell, Jr., and other colleagues, and the more recent efforts of younger scholars including Charles Rosenberg, Martin Kaufman, and Barbara Rosenkrantz, to make another major contribution: a synthetic and interpretive history of American medicine.

The book begins with sketches of Indian medicine and early colonial diseases followed by more detailed accounts of the variety of practitioners and therapies, the origins of a profession, medicine during the Revolution, and Benjamin Rush. Readers will notice with interest that no generation in this country's history has had a monopoly on ambivalent feelings toward doctors and medicine. A second group of chapters is devoted to major developments between 1800-65: the failure of licensing regulation, the rise of competing irregular sects, and the proliferation of proprietary medical schools. Two bright spots in this troubled era were the emergence of the organized public health movement and certain refinements in surgery. In the area of gynecology and obstetrics, however, Southern physicians showed a less than laudable inclination to try new and hazardous procedures on slave women. Later chapters place events of the past century in perspective by analysis of the principal factors influencing medical education, licensure, and organization together with an evaluation of trends in medical research, practice, and public health. The author concludes with a careful appraisal of medicine today.

Duffy concedes a certain empathy for the medical profession, but he does not abstain from temperate criticism of its shortcomings. Regarding therapeutics, he suggests that the practice of polypharmacy today is as deplorable as it was in the time of Benjamin Rush and that the historic advancement of surgery hardly warrants unnecessary performance of surgical procedures. He also scores

organized medicine for failure to sponsor needed licensure reform and for its obtuseness about the realities of medical care in our time. Refusal to accept its responsibilities may bring to pass that which the profession fears most. On the other hand, Duffy defends the medical establishment from attack by those who charge it with playing God: "The question as to whether or not man should play God has already been answered; the real issue is how well we do it" (p. 325).

This thoughtful and timely book is a most important contribution to the history of medicine. It should be read by scholar, physician, and layman alike.

JOHN H. ELLIS
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JAMES BORDLEY III and A. MCGEEHEE HARVEY. *Two Centuries of American Medicine, 1776-1976*. Philadelphia: W. B. Saunders Company. 1976. Pp. xv, 844. \$19.76.

According to the authors, both professors of medicine, their intention was to provide "an account of the extraordinary advances in medical education and in the prevention and treatment of diseases that have taken place" from 1776 to 1976. In the process, they intended to explain how America became a leader in medical research.

In general terms, James Bordley and A. McGehee Harvey portray early American physicians as "good Samaritans" with no scientific base and with ineffective treatments based on false premises. By the 1870s and 1880s, the scientific and bacteriological revolutions broadened the scientific base, substantially improving medical practice. The physician became a healer with precise diagnosis and adequate treatment. The proliferation of scientific knowledge encouraged the development of medical specialism and the decline of the general practitioner of the past.

After World War II, the government began to support medical and scientific research on a large scale, in the hope of conquering cancer and cardiovascular disease. More recently the government has shifted its interest to problems of health delivery. Meanwhile, the American Medical Association, the voice of American medicine, has abandoned its earlier role of reform advocate and has become a conservative force protecting the interests of the physicians rather than the health of the public.

The book is divided into three main sections. The period from 1776 to 1876 is covered in 100 pages, 1876 to 1946 in 270 pages, and 1946 to 1976 in 400 pages. The organization indicates an emphasis on medical science, rather than on the social history of medicine, which is largely neglected.

In spite of an understandable overall organization, when it comes to specific developments, the book is poorly organized. A good case in point is the description of the formation of the AMA. Early in the book it is related to the need to reform medical education, but later it is described as a response to the growth of medical sects, the elimination of medical licensing laws, and the declining public image of the physician. The casual reader will likely come away with a simplified view as a result of these problems of organization.

A much more serious problem, however, is the fact that the authors missed some of the best and most revealing studies by competent historians. The sections on preventive medicine, for instance, would have benefited from the use of John Duffy's two-volume study of public health in New York City, and from Barbara Rosenkrantz's study of the Massachusetts State Board of Health. Similarly, although the book devotes a great deal of space to medical education, it does so without making use of William F. Norwood's volume on medical education before the Civil War, or from any of his or John Field's survey articles on the subject. This reviewer's book, of course, appeared too late to be utilized by the authors. The coverage of the Civil War was terribly brief, and it could have benefited from the use of the classics by G. W. Adams and H. H. Cunningham, as well as the excellent article by Gert Brieger on therapeutic conflicts during the war. It is difficult to understand how any study of American hospitals can be written without consideration of the ideas and facts developed by Gerald Grob and David Rothman in their separate studies. Finally, Elizabeth Etheridge's excellent study of the attack on pellagra is neither cited nor used.

It seems apparent from the omission of so many leading sources that the authors place their emphasis on the history of medical research, rather than on the history of American medicine. Finally, the authors fail to come to grips with a major inconsistency in the story: the failure of American medicine to keep up with other nations in terms of the delivery of health services.

In spite of these problems, the book does have its merits. It provides a great deal of information that is largely unavailable elsewhere, or at least is unavailable in one volume. The book is especially useful for its extensive coverage of developments in twentieth-century medical science, which undoubtedly reflects the authors' competence as professional physicians and medical educators.

MARTIN KAUFMAN
Westfield State College

MARTIN KAUFMAN. *American Medical Education: The Formative Years, 1765-1910*. Westport, Conn.: Greenwood Press, 1976. Pp. x, 208. \$12.95.

Martin Kaufman attempts to provide a synthesis of the history of American medical education and to trace the development of the reform movement which culminated in Abraham Flexner's 1910 report on medical education and the subsequent disappearance of most of the marginal medical schools. As several scholars have noted, despite promising beginnings in the 1760s, American medical education deteriorated after 1800. Schools proliferated; standards plummeted. The medical profession itself came under public attack, and heterodox systems of medicine such as homeopathy and Thomsonianism won converts by the thousands. Kaufman reviews these developments and carefully outlines the emergence of reformist demands from 1827, when a number of state medical societies sent delegates to a convention at Northampton, Massachusetts, through the establishment of the American Medical Association in 1846, the American Medical College Association in 1876, and the Association of American Medical Colleges in 1890. All of the reformist demands had a similar ring: raise admission requirements and lengthen the term of medical education. For the most part the results can be summarized by the awkward but accurate title of one of Kaufman's chapters, "The Situation Deteriorates Further."

Change started in the 1890s. The model institution was the medical school at Johns Hopkins University, whose endowment made it independent of student fees and hence capable of cracking down. Johns Hopkins had powerful allies in the American Medical Association's Council on Medical Education; the latter initiated overtures to the Carnegie Foundation which resulted in the Flexner report. In addition, by 1900 orthodox and homeopathic practitioners were cooperating with each other, thus removing one longstanding roadblock to reform, while genuine advances in medical science made the Hopkins model visibly superior to alternatives.

Kaufman's habit of viewing matters through the eyes of reformers seriously limits the value of his study, for reformers rarely saw the heart of the issue. Low educational standards during the nineteenth century reflected the meager resources of most students and the meager compensation of most practitioners. Schools raised standards only at the risk of pricing themselves out of the market. For this reason I find it implausible that change came after 1900 because reformers desired it or because reformers now had a few allies. Flexner himself noted that the drastic reduction in the number of American medical schools between 1910 and 1924 occurred particularly in the South and West, areas in which neither reformers nor their allies seem to have been especially powerful. Perhaps what is needed is less the sort of rehashing of

familiar material that Kaufman provides than an analysis of the social and economic determinants of the transformation of the medical profession.

JOSEPH F. KETT
University of Virginia

HARRY EMERSON WILDES. *William Penn*. New York: Macmillan Publishing Company. 1974. Pp. lx, 469. \$14.95.

HANS FANTEL. *William Penn: Apostle of Dissent*. New York: William Morrow and Company. 1974. Pp. xiv, 298. \$8.95.

Both Harry Emerson Wildes in his *William Penn* and Hans Fantel in *William Penn: Apostle of Dissent* have attempted to treat a most elusive subject. Each succeeds to some extent. Wildes' more comprehensive work provides in-depth examinations of the founder of Pennsylvania, his family, and his circle. In fifty chapters and nine appendixes, he probes into William Penn's life and interests. This is one of the most detailed biographies of Penn now in print.

Wildes is at his best when setting the scene and detailing pertinent background information. For example, he demonstrates quite clearly the disinterest of the Pennsylvania Assembly, dominated by Quaker merchants, in the importation of black slaves into the province. Laws on that subject, passed by Friendly legislators, were reversed in London. Not even idealists might interfere with the rights of English merchants. Personality studies by Wildes add another dimension to this work. In the finest description of William Penn since the superb treatment given by Mary Maples Dunn, Wildes provides a balanced account. Penn, the idealist spokesman for the Society of Friends, seemed to prefer to associate with persons of quality. Wildes gives Penn credit for the finest colonial constitution, the Charter of 1701.

Perhaps Wildes' most impressive description is his sympathetic treatment of an aging Penn who realizes the high cost of his experiment. Troubles brought on by a wayward son and the Philip Ford claims might have been diminished by a more alert proprietor. The book is well done, though hardly one which renders all other biographies of Penn obsolete.

Hans Fantel, in his *William Penn: Apostle of Dissent*, does achieve his goal, a popular biography on a theme of current concern. Yet striking new insights into the chief Dissenter of his time do not follow. Fantel renders William Penn a very human person in this word portrait. He finds contemporary expressions about the eccentricities of the Quaker Proprietor. The author also thoroughly

examines the influence of older friends and advisors upon the youthful William Penn.

Neither Wildes nor Fantel attempts any real assessment of the effects of several visits to the Rhineland upon the youthful Penn. There he saw energetic, ambitious farmers frustrated by archaic feudal land regulations. Later advertisements for Rhineland farmers to join his Pennsylvania experiment indicate the extent of his concern. Fantel's study of Dissent gives little coverage to this pragmatic pressure on young Penn. Moreover, the author debates whether a theology of love is radical or conservative, but comes to no conclusion.

Irritating to scholarly readers of *William Penn: Apostle of Dissent* is Fantel's failure to provide precise identification of direct quotations, though, of course, the author does warn the reader that he speaks to a popular audience. Bibliographies and appendixes in Wildes' *William Penn*, are superb; Fantel's basic bibliography is probably sufficient for the general reader.

WILLIAM T. PARSONS
Ursinus College

PAUL R. LUCAS. *Valley of Discord: Church and Society along the Connecticut River, 1636-1725*. Hanover, N.H.: University Press of New England. 1976. Pp. xiv, 275. \$12.00.

Historians have long denied that the Massachusetts Colony was synonymous with New England (as many Bay colonists assumed), but a paucity of studies on the other colonies has hampered accurate comparison and differentiation. That has been especially true of seventeenth-century Connecticut, which has languished in the shadow of its larger neighbor. Paul Lucas has now brought the River Colony (more precisely, the colony and adjacent western Massachusetts) into the limelight.

Valley of Discord makes three important contributions to a more sophisticated understanding of Puritan New England. First, it demonstrates the virulent contention within and among Connecticut's congregations, a condition that began early in the colony's career and persisted with only minor intermissions through the Great Awakening. Second, the book traces Connecticut church polity from its initial Congregationalism, dominated by the clergy and with membership restricted to those with conversion experiences, to its eventual quasi-Presbyterianism, dominated by the laity and with sainthood measured by moral rectitude instead of by saving grace. Third, *Valley of Discord* clarifies, as no previous study has, the several stages of Solomon Stoddard's thought and the nature of his battle with the Mathers.

Much of Lucas argument is revisionist. His em-

phasis on social discord tacitly challenges recent claims (e.g. T. H. Breen and Stephen Foster in the *Journal of American History*, LX [1973]) that early New England enjoyed remarkable stability—a clash of interpretations not adequately explained by differences in geographical focus. Lucas also lends new credence to the old notion of a “declension” in late seventeenth-century Puritanism (recently denied by many scholars, including Robert G. Pope in the *Journal of Social History*, III [1969–70]); Lucas contends that as most towns grew in population the proportion of church members failed to keep pace with the non-churched and that *piety*, as distinct from morality, declined. Lucas’ explication of Stoddard’s writings corrects many misunderstandings perpetrated by Perry Miller and others.

The book has a few shortcomings. Some are conceptual: the Connecticut Valley as a geographical entity is not always meaningful in a discussion of religious ideas and forms, and the fixation on discord obscures evidence of harmony. Other problems are stylistic. Lucas’ prose is sprightly but often redundant, and in an apparent attempt to invigorate an inherently dreary topic—to all but devotees of intramural squabbling—he occasionally indulges in overstatement: “ecclesiastical chaos” (p. 31), “startling innovation” (p. 39), and “Connecticut’s agony” (p. 42) are cases in point. Too much important material is buried in the notes, unfortunately placed at the back of the book. Last and not least, the author and publisher have permitted an unconscionable number of typographical errors. *Valley of Discord* deserved more careful editing because it merits thoughtful reading.

ALDEN T. VAUGHAN
Columbia University

ESTELLE F. FEINSTEIN. *Stamford from Puritan to Patriot: The Shaping of a Connecticut Community, 1641–1774*. Stamford: Stamford Bicentennial Corporation. 1976. Pp. iii, 236. \$5.95.

Stamford from Puritan to Patriot is a tremendously useful book by the author of *Stamford in the Gilded Age* (1973). In a short work Estelle F. Feinstein has dealt with settlement patterns, the social structure, office holding, town-colony relations, intratown centripetal and centrifugal geographic forces, secularization, consensual and conflict relations, and demographic movements. Feinstein does not pursue her data as far as did Charles Grant in his study of Kent, nor does she attempt the cliometric tour de force of Linda Bissel’s unpublished study of Windsor. But in a style written to be read she touches base at every point and enlists sufficient data for sound generalization.

Feinstein depicts Stamford as tied together by the customary New England trinity of open fields, Congregationalism, and the town meeting. Open field agriculture she finds effective for sixty-five years, abandoned only after all town lands had been privatized—a counter example to the growing number of towns shown to be non-nuclear and individually farmsteaded from their inception. Ecclesiastical cohesion remained an ideological sticking force until the hiving of three new parishes and the intrusion of Anglicanism in association with the Great Awakening. The town meeting remained central as long as it dealt with land and religious affairs, atrophying from 1710 to 1770, and returning to prominence with the politicization of the Revolutionary era. Family tribalism, within the context of a deferential oligarchy, carried the town in unity and tranquility through periods of religious and geographic fragmentation.

Feinstein challenges the accepted view that Connecticut politics during the eighteenth century was divided into eastern radicals and western conservatives by demonstrating that Stamford—in the west—though generally conservative, was dominated by radicals on the Susquehannah, Stamp Act, and independence issues. Her suggestion that the establishment of numerous outlying school districts underlay the hiving of new parishes is less well supported. Few scholars accept her view that the Saybrook Platform brought religious uniformity to Connecticut congregationalism.

Feinstein’s excellent work is a worthy companion to Grant’s study of Kent. One hopes that its sponsorship by a local historical society—and extraordinarily shoddy editing—will not deter professionals from serious attention to this sound, tightly constructed scholarly work.

CHRISTOPHER COLLIER
University of Bridgeport

EDWARD M. COOK, JR. *The Fathers of the Towns: Leadership and Community Structure in Eighteenth-Century New England*. (Johns Hopkins Studies in Historical and Political Science.) Baltimore: Johns Hopkins University Press. 1976. Pp. xvii, 273. \$12.95.

In *The Fathers of the Towns* Edward M. Cook, Jr., tests one of the central issues of eighteenth-century American historiography during the past twenty years—whether or not New England towns were egalitarian communities. He concludes, after analyzing the records of office-holding in seventy-four towns (thirteen percent of the total), that the pattern was mixed. The poorer agricultural towns were indeed egalitarian, Cook argues, with broad access to high office, short terms, and no substantial differences of wealth, family, religion, or

education between selectmen and the majority of their townsmen. In contrast, Cook finds that commercial and political centers possessed more highly stratified social and political structures, and relatively deferential, elite-dominated office-holding patterns. Between the two extremes, the author shows that behavior varied within this spectrum. These conclusions will not surprise scholars familiar with public life in eighteenth-century New England, but they are nevertheless significant. Cook's exhaustive research answers some controverted questions virtually definitively; and his evidence moves the consideration of the character of community life and colonial politics to a new plane. *The Fathers of the Towns* is an important book.

Cook is a meticulous scholar who presents his evidence directly and systematically. He establishes clearly that most selectmen and representatives served lengthy apprenticeships in lesser offices, rising to the pinnacle of local office-holding in their forties. After serving a half-dozen years, they frequently returned to lesser town offices. In some towns, Cook finds, former selectmen did such chores as sweeping the meeting-house without any loss of esteem. In the poorer farming towns, where "ability"—a combination of personal talents and free time—seems to have been most important, Cook conveys the sense of a genuinely democratic political order. Yet in port towns, county seats, and other commercial centers, Cook explains that wealth and social prominence were influential in the distribution of major offices. The "best people" gained high office at earlier ages and stayed in them for longer terms. Moreover, the county and colony political elites were heavily drawn from this circle. Here, Cook argues, deferential politics flourished. As a result Cook's New England harbors both deference and democracy.

Cook believes that much of the egalitarianism was made necessary by the local available leadership pool. When men of wealth and family connections did enter a poor town they were likely to be rapidly thrust forward into leadership, suggesting a disposition for deference everywhere. Cook hints, as Clifford Shipton and others have done, that high office was a prestigious form of tax burden. Cook's evidence suggests that class-based rivalries were normally absent from eighteenth-century local politics and that the contradictions we see between deference and democracy, perhaps even the terms themselves, are anachronistic.

Moving beyond Van Beck Hall, Jackson Main, Robert Zemsky, and Michael Zuckerman, Cook argues persuasively that "central place theory" furnishes a useful model for classification of towns, and he presents his own five-cell "typology." Al-

though Cook does not explicitly argue the case that eighteenth-century New England was becoming more stratified and less egalitarian, his evidence points in that direction, since it appears that many towns were moving toward the relatively elite, oligarchic type. Regrettably Cook never estimates how many New England towns or what percentage of the population fell into each category at any given time, so for the present the short answer to Cook's key question—"was town politics democratic or deferential"—is "both."

RICHARD D. BROWN
University of Connecticut

DOUGLAS GREENBERG. *Crime and Law Enforcement in the Colony of New York, 1691-1776*. Ithaca: Cornell University Press. 1976. Pp. 259. \$15.00.

Douglas Greenberg's objective in this prize-winning study of crime in colonial New York is to examine the connections between deviant behavior and social environment, and the questions he asks are important ones. He wants to know the ethnic identity, sex, and urban or rural characteristics of those charged with crime, whether certain types of crimes were associated with specific groups, and how defendants were treated by New York's criminal justice system. To find the answers he has analyzed 5,297 criminal cases. Because he was able to obtain a complete run of New York County General Sessions records from 1691 to 1775 the sample is, as he concedes, "heavily biased" in favor of New York City.

Greenberg finds that most criminal defendants in New York were male, white, and of English ancestry. Of the nearly 5,300 cases scrutinized 90.1 percent involved men and 9.9 percent involved women; in only 7.4 percent of cases were the defendants black, mainly slaves, though blacks composed from 11 to 15 percent of New York's population in the period under study; and 73.4 percent of those accused of crimes were of English extraction. Women were far more likely than men to be prosecuted for crimes of theft, which is explained by the female defendants' frequent association with houses of prostitution where thievery was rampant. Riots and breaches of the peace occurred most often in rural areas, owing to the outbreak of land riots in New York's late colonial history. After mid-century, thefts and crimes of personal violence (assaults, rapes) rose sharply in the city.

Greenberg acknowledges that the attempt to correlate crime with ethnic identity is fraught with difficulty. The heavy preponderance of defendants with English surnames is not unexpected in view of the steady flow of New Englanders and English to New York in the eighteenth century as well as the anglicized spellings adopted by many New York

City Dutch and French families. But Greenberg is troubled by the low rate of Dutch criminality (13.1 percent), though the problem may lie in his assertion that New York was still 50 percent Dutch at the end of the colonial period. Even if this were true, and it is highly improbable, the Dutch were concentrated in Kings and the Hudson River counties and the disproportion of New York County court records alone could account for the disparity. Only a county-by-county analysis that related Dutch criminality to Dutch population estimates would provide an answer to this perhaps insoluble problem.

Patterns of criminality, as Greenberg observes, often reveal as much about the enforcers of the law as they do about those against whom the law is enforced. Two of his statistics illustrate this: over 87 percent of the female defendants were unmarried, and when blacks were brought to trial they were far more likely to be convicted than were whites. Surely these figures reflect in part the social assumptions and fears of eighteenth-century New Yorkers.

In the quarter-century before the Revolution the number of criminal cases rose sharply, and crowded dockets led to delayed and erratic enforcement of the law. Thus the colonial system of justice failed at the end "to deal effectively with crime in a society which was in the process of fundamental transformation." This excellent and detailed study helps us to understand why.

PATRICIA U. BONOMI
New York University

HAROLD E. DAVIS. *The Fledgling Province: Social and Cultural Life in Colonial Georgia, 1733-1776*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1976. Pp. xi, 306. \$16.95.

This is an excellent book, a significant contribution to the historical literature of colonial Georgia. The author has sought, successfully, to do for Georgia what Carl Bridenbaugh has done for the colonial cities. Harold E. Davis treats his readers to a delightful analysis of Georgia's social structure, the daily routine of its inhabitants, the occupations which provided Georgians with their livelihoods, and the culture, religion, and education that were available to people living in a "fledgling province." His findings, presented in an enjoyable style, are based on an impressive research effort. The book's major conclusion is that Georgia, despite a shaky beginning and some trying times under the Trustees and Governor John Reynolds, was "squarely in the mainstream of contemporary American experience" by 1776 (p. 3). Taking a lesson from Michael Kraus, who years ago empha-

sized the things the various American colonies had in common, Davis focuses on those things that made Georgia and Georgians like other American colonies and their people. He is convinced that Georgians, because of a common language and cultural heritage, "considered the interests of Georgia and America as one" in 1776 (p. 256). Davis also argues that Georgia was not as culturally deprived as some writers have contended.

Even good books have flaws, or more accurately perhaps, do not cater to the preferences of the reviewer. The author frequently uses "trustees" and "proprietors" interchangeably, but the Georgia Trustees were not proprietors in the sense that William Penn and Lord Baltimore were. Technically the Trustees should not be called proprietors.

Although it is a good chapter—informative and basically sound—the one on religion contains several minor inadequacies. Judaism receives superficial treatment, and the reader is left with some erroneous impressions regarding the respective number of Sephardim and Ashkenazim among Georgia Jews. Also, Bethesda, George Whitefield's orphanage near Savannah, is referred to as a Methodist enclave, when in fact it was a hotbed of Calvinist theology. Whitefield was called a Methodist all his life, but he was not one except during his college days at Oxford. From 1740 on he espoused a modified version of Calvinism, and the young men who entered the ministry from Bethesda tended to become Baptists, not Methodists. Moreover, Davis speculates that Daniel Marshall, the Separate Baptist evangelist, was arrested near Augusta for disturbing the peace. Actually, Marshall was arrested for violating the Establishment Act, because a constable, supported by an Anglican clergyman, misconstrued that law.

But the strong points of this book greatly outnumber its weaknesses, and it should be enthusiastically received. In addition to its solid research and readable style, the book offers a most helpful bibliographical essay and a good index. Those who seek to understand colonial Georgia can come to this work with great expectations, and they will not be disappointed.

DAVID T. MORGAN
University of Montevall

MARIE-ANTOINETTE MENIER *et al.*, editors. *Correspondance à l'arrivée en provenance de la Louisiane*. Volume 1. (Interventaire des Archives Coloniales.) Paris: Archives Nationales. 1976. Pp. 479. 120 fr.

From the French National Archives comes a bicentennial gift to the American people, the first of two volumes inventorying its C¹³ series. The series is composed of the correspondence of the *Secrétaire*

d'État de la Marine concerning the Colony of Louisiana, with this issue detailing the entries in C¹³a 1 to 37, or those from 1678 to 1753. Each item in the listing receives a short, accurate summation and an indication of its length. The framework is basically chronological, but with some topical alignment within each period as the communiqués between the colonial governor and the French commissioner are placed first, followed by those of lesser officials, and finally miscellaneous documents.

The material included in the C¹³ series is an invaluable resource on the commercial, diplomatic, military, and political adventures of France in the New World. This early section also holds a mine of underused information on the institution of slavery, the development of the Catholic Church, and relations of the Indian tribes, both among themselves and with the Europeans. In addition, it contains important insights into a broad range of topics from natural disasters, medicine, and social deviance, to the colony's need for stable families and wine. Fortunately, financially stricken Americanists are not forced to travel to Paris to use this material, for the Library of Congress has microfilmed the series and deposited copies at the University of Southwestern Louisiana, Loyola University of New Orleans, Memphis State University, and the Mississippi Department of Archives and History.

If the index promised in the second volume is competent, these compilations should prove a handy guide into almost every aspect of colonial Louisiana life. For the C¹³ series, they supplement Nancy Surrey's more extensive *Calendar of Manuscripts in the Paris Archives Relating to the History of the Mississippi Valley to 1803*, a rare work that in proper bicentennial spirit should be accorded more credit than is given it in this new treatment.

FREDERICK STIELOW
Grinnell, Iowa

JOHN PRESTON MOORE. *Revolt in Louisiana: The Spanish Occupation, 1766-1770*. Baton Rouge: Louisiana State University Press. 1976. Pp. xiv, 246. \$10.95.

This well-researched monograph, based primarily on archival material, treats the French revolt in Louisiana during the first years of Spanish control following the Seven Years' War. For a long interval Versailles had neglected the province so that it had enjoyed some degree of commercial autonomy. Following the cession in 1763 more than three years were to pass before a Spanish governor, Antonio de Ulloa, arrived with a force of only ninety soldiers. The Spanish hoped to recruit more men from the French garrison! Short of men and money and uncertain of the attitude of the new

subject, Ulloa refrained from formally taking possession but attempted to rule jointly with his French predecessor. His failure to establish de jure sovereignty for Spain offered a pretext of legitimacy to those who later overthrew him. However well-intentioned Ulloa was, he gave the impression of being a haughty *hidalgo*. His condemnation of the favorite pastimes in the province, gambling and tippling, may not have endeared him to the rank and file of the populace. For the better folk, the merchants and traders, his attempt to limit their commerce to the Spanish empire was disastrous. What compounded his difficulties was the chronic shortage of money—a shortage made all the more serious by the parsimony of the Spanish crown and the unfortunate timing of a reorganization of the finances of Mexico and Cuba. Ulloa was hard pressed to build defenses against the British in the Floridas, to cement alliances with the Indians, or even to maintain the almost penniless Acadian immigrants supported by governmental subsidies.

In the fall of 1768 a group of discontented traders and landowners, concluding their economic interests were imperiled by the Spanish commercial system, instigated a revolt. They may have hoped to force the hand of the ministers at Versailles before a strong Spanish garrison, then being assembled at Havana, arrived. The ringleaders plied the local population, including the Acadians and recent German immigrants, with enough propaganda and good Bordeaux wine to muster several hundred militia. Ulloa, with only a hundred or so men at his command, capitulated and departed the city for Havana. The revolt brought no relief; foreign trade did not revive, attempts to raise money failed, and Choiseul in Versailles, to whom the insurgents turned, refused to aid the rebels in the face of Spanish determination to reclaim the province. Disillusioned, despairing, and finally anxious, the Creoles offered no resistance when General Alexander O'Reilly with 2,000 troops occupied New Orleans in the summer of 1769. O'Reilly executed five of the ringleaders for treason, offered amnesty to those who took an oath of allegiance, and quickly integrated Louisiana into the Spanish empire in America.

John Preston Moore sees the revolt as an experiment in virtual self-rule under nominal French sovereignty, one that resembled the revolt a few years later on the Atlantic seaboard, in that both were economic in cause and manifested an inherent localism. Like the English colonists, the Creoles cherished the rights and privileges gained over the years of imperial neglect. Those more familiar with the origins of the American Revolution might doubt the primacy of economic causes in the motivation of the English colonists. And the

evidence Moore himself presents for Louisiana is that the great majority of the Creoles were not seeking to establish a "separate and equal station" among the "powers of the earth," but to rejoin, however, tenuously, the French empire.

JACK SOSIN
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Lincoln*

HENRY F. DOBYNS. *Spanish Colonial Tucson: A Demographic History*. Tucson: University of Arizona Press. 1976. Pp. x, 246. Cloth \$11.50, paper \$5.95.

How does one account for the paucity of scholarship on Hispanic and Native American themes in Arizona history? Unlike its Borderland compadres, California, New Mexico, and Texas, Arizona has escaped both the serious and the popular attention lavished on its neighbors. In an effort to redress this imbalance, anthropologist Henry F. Dobyns has examined the dynamics of ethnic diversification in Tucson during Spanish colonial rule, a period of at most seventy years which ended in 1821. Within this brief span there occurred the building of a mixed culture in the desert Southwest marked by the diligent effort characteristic of Spanish colonization in frontier environments.

Dobyns considers the military, ecclesiastical, and biological transformations wrought by the Spanish as they endeavored to proselytize and checkmate Apache incursions on the northern flank of New Spain. Relying primarily on archival sources, Dobyns constructs a detailed narrative of events which shows that the Spanish impact in these first two realms differs little from what one might expect. Thus, Tucson's appearance in the Borderlands literature will be welcome as a case study.

As viewed by an anthropologist, the biological implications of colonization will also intrigue the reader. Successful in their efforts to stabilize the Native American population, Spanish Franciscans were able to prevail upon them to adopt European modes of dress. The author suggests that as this change proceeded in the absence of better standards of personal hygiene, the decimation of Tucson's Indian population followed. Indeed, Dobyns shows that the mission program succeeded only in bringing in enough fresh converts to replace those who had expired without reproducing themselves in the mission community. Dobyns thus joins those who have empirically questioned the success of the mission as a frontier institution, particularly in light of the grim statistics fostered by that environment. Parenthetically, even at California's model Carmel Mission, a surprisingly large number of Native American deaths occurred during that establishment's duration of seven decades.

Less well-served, however, are the treatments of the Tucson presidio, founded in 1776, and the role of the army of New Spain in ethnic diversification. Although the presidio "determined the specific location of the modern city and set its pattern of early urban development," little else is said of its role, not only as a catalyst of physical change and growth, but as a partner in the sometimes contentious collaboration with its neighboring mission. Similarly, despite collecting extensive documentation on the presidial force, Dobyns does not satisfy in his treatment of the frontier army and its role in ethnic diversification. Itself the product of *mestizaje*, the presidial garrison could have greatly enriched a discussion of Tucson's ethnic blend. There remains ample room to expand this study's scope as well as to enrich its perspectives with contemporary Borderlands scholarship; let us hope that the author has begun a project which will bear additional fruit.

DANIEL J. GARR
San José State University

BERNARD BAILYN. *The Ordeal of Thomas Hutchinson*. Cambridge, Mass.: Belknap Press of Harvard University Press. 1974. Pp. xx, 423. \$12.50.

Bernard Bailyn has written a bittersweet account of Thomas Hutchinson's role in the coming of the Revolution. He finds Hutchinson in many ways a sympathetic character. An intelligent, forthright politician and historian, a man of admirable rectitude, Hutchinson failed in his attempt to contain the American Revolution and died maligned by his enemies and shunned by the British government he had served so well. In explaining Hutchinson, a loser in history, Bailyn hopes to shed a brighter light on the meaning of the Revolution.

Bailyn concentrates on Hutchinson's public life after 1760, when, allied with Francis Bernard, he began building his remarkably successful political career. But his success only masked the ruin that lay in the future. Caught between the radical opposition in the colony and a series of British ministries that seemed bent on wrecking the empire by incompetence and bullheadedness, he strove with a monumental patience to enlighten the radicals and temper ministerial policy. He failed at both. Although he opposed all of the measures between the Stamp Act and the Coercive Acts that did so much to provoke the revolutionary turmoil, the radicals marked him as an instrument of tyranny who had conspired to erase American liberty. Until Lord Dartmouth came to the colonial office, the ministries largely ignored his advice.

Yet in Bailyn's view, Hutchinson was no paragon of worldly wisdom. He exhibited ideological myopia, an insensitivity to the winds of change

that crippled him for the task of maintaining the bonds of empire. Hutchinson suffered from that endemic disease of modernity: he was behind the times. He represented the prudential politics of the Walpolean settlement. Authority, class interest, political manipulation, and office-seeking constituted the bases of public order. When faced with the ideological striving of the revolutionaries, the fanatic determination to cleanse the British constitution of corruption, Hutchinson first appealed to reason and then to interest. Neither proved effective because the revolutionary age had heightened the American appetite for a different sort of reason and rejected the little compensations of office and money that in other times assuaged the craving for perfection.

Nor does it seem that Hutchinson was theoretically consistent. His theory of the state, in so far as he had one, came from Locke. Contractual government should not have been easily reconciled with the world of status Hutchinson had come to love as an imperial politician. Moreover, when he went to England in 1774, Hutchinson manifested all the symptoms of virulent Whiggery. The sycophancy, superciliousness, and corruption of the ruling class appalled him. But, as Bailyn makes clear, all of this was sensibility. Hutchinson was an intelligent man, but no theoretician. He could go only so far in dissecting the ills of his age, and this was his downfall.

For all Bailyn's sympathy for the personal tragedy of Thomas Hutchinson, his interpretation of the Revolution comes down firmly on the Whig side. The American radicals may have been fanatic and frequently unpleasant, but they held to humane and progressive principles. It was the radicals who sought to meliorate the human condition, to transcend the inhibitions of time and usage. Hutchinson may have been a good man, but he represented an antique world that deserved to die. That Bailyn has been able to suspend his own ideological loyalties to paint so poignant a portrait of a man so deeply reactionary is a tribute to his integrity as a biographer. Still, one can only wonder how Hutchinson might have fared at the pen of a biographer less firmly gripped by the verities of modernity.

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THOMAS O'BRIEN HANLEY, editor. *The John Carroll Papers*. Volume 1, 1755-1791; volume 2, 1792-1806; volume 3, 1807-1815. Notre Dame: University of Notre Dame Press. 1976. Pp. li, 551; liv, 543; liii, 517. \$75.00 the set.

In the sixty years spanned by these volumes (1755-1815) John Carroll wrote hundreds of letters,

the first to his cousin, Charles Carroll of Carrollton, and the last to Elizabeth Seton. Nor were these all, for the archives of the Archdiocese of Baltimore contain a considerable number of other Carroll letters which for some unaccountable reason were omitted. In addition to letters the final volume contains a hundred pages of sermons, memoranda, notations, and fragments. It is a rich array, which sheds its principal light on the problems of the Catholic community during the revolutionary generation and the early national period. Yet Carroll's vision extended beyond ecclesiastical horizons and revealed a lively interest in public affairs; for example, education, war, slavery, the Federalist Party, and successive presidential administrations, with special praise for the virtues of George Washington.

If ever a religious community needed a leader endowed with tact, sound judgment, and common sense, it was the Catholic minority of about 30,000 over whom Carroll was appointed as bishop in 1789. These letters bear convincing testimony that during the next quarter-century he demonstrated these qualities to a marked degree. This Maryland-born former Jesuit had little or no flair, but he possessed a striking endowment of what told in a far more positive and enduring way in the lives of his spiritual charges. If in 1779 Carroll proved to be too optimistic about "the fullest & largest system of toleration" adopted in "almost all the American states" (1:53), his assessment of contemporary trends was normally accurate and judicious. In his correspondence he repeatedly insisted that his coreligionists should remain constantly alive to the sensibilities of the non-Catholic majority among whom they lived. "Being admitted to equal toleration," he remarked in 1784 to a fellow priest, "must we not concur in public measures, & avoid separating ourselves from the Community?" (1:158). It was a policy from which Carroll never departed, for he was just as public-spirited at the age of eighty (1815) as he had been when he assumed his episcopal duties in 1790.

All students of American religious history will welcome these volumes, and especially those whose interest centers on Catholicism. It is all the more regrettable, therefore, that the editing leaves so much to be desired. The footnotes show a woeeful ignorance of American Catholic history, a fact that could be abundantly illustrated if space allowed. The errors carry through from a wrong date for the founding of Saint Mary's Seminary in Baltimore (1:xliv) through four errors in a single paragraph (2:lii) to wrong names for Elizabeth Seton's daughters (3:369). If scholars cannot always expect the virtually flawless editing of the late Charles Stephen Dessain in *The Letters and Diaries of John Henry Newman*, they have a right to anticipate that an editor will demonstrate a gener-

ally informed knowledge of the historical background of correspondence such as is found here. But although students should be alerted to the faulty editing of Archbishop Carroll's papers, they will find the documents themselves now available in a generally acceptable form.

JOHN TRACY ELLIS

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GEORGE ATHAN BILLIAS. *Elbridge Gerry: Founding Father and Republican Statesman*. New York: McGraw-Hill Book Company. 1976. Pp. xviii, 442. \$19.95.

This is a most literate and urbane biography, written by a scholar who read and thought about his sources. Elbridge Gerry remains one of the most misunderstood and controversial of America's Founding Fathers: a signer of the Declaration of Independence who refused to sign the Constitution; a supposedly wrongheaded negotiator in the XYZ affair; and a thoroughly partisan, even bigoted governor of Massachusetts in the years before the War of 1812. Indeed, when one thinks of Gerry, the instant association is "gerrymander."

George Athan Billias for the first time effectively places Gerry in proper historical perspective in all of the above situations and in others as well. Born to the "codfish aristocracy" of Marblehead, Massachusetts and Harvard-educated, Gerry had a commitment to the American cause that never wavered after 1763. Billias finds that Gerry's "republicanism" was the overwhelming motivating force of his political career. Defined in depth throughout the book, that republicanism made Gerry antimonarchical, antimilitaristic, and anti-party up to 1800. In a positive sense "it constituted a set of moral ideals" (p. 332) that committed Gerry to a "mixed constitution" (p. 160), a "natural elite" (p. 63), and a belief in the "moral virtue" (p. 68) of the American people. It may be said of Gerry that his fear of "democratic" excesses was exceeded only by his loathing for monarchists and militarists. Here was a man who helped shape the Declaration and the Constitution, who served ably in the first two Congresses and as a minister to France, governor of Massachusetts and vice-president of the United States, and yet is remembered only as "a political crank . . . given to unpopular positions, and constant only in his inconsistency" (p. 1).

As this biography catches on with historians, Billias will have changed all that. The book offers a series of reinterpretations of Gerry's many roles. Billias' Gerry emerges as a consistent opponent of the military from 1776 through 1800, and a man who made his way politically despite his stubbornness because he was a "person of great personality and charm." According to the author, Gerry was not an extreme Antifederalist at the Constitutional

Convention of 1787 and thereafter. The fact that survives is that Gerry was the only attending delegate who refused to sign the document. Yet Billias offers ample evidence that this Founding Father was a moderate whose fears for the sovereignty of the states and the liberties of the people, and in particular whose ever-present aversion to "incipient military despotism" (p. 202), drove him to press his compatriots for greater guarantees of liberty. "Without the arguments of Gerry, Mason, Randolph and Martin," Billias notes, "the Constitution would have emerged a far different document" (p. 204).

There are occasional interpretations that are marred by what I perceive as a quality of special pleading. For example, one can make a good case that Gerry in the 1790s was less the author's principled opponent of party than he was a trimmer in the worst sense of that eighteenth-century word. His correspondence with Jefferson suggests that, as does the fact that a few months after supporting Adams for the presidency in 1800 Gerry ran for governor of Massachusetts as a Republican.

These are quibbles over interpretation of evidence, however. There is an overriding objectivity about this book that is its chief hallmark, along with the author's ability to place Gerry in proper historical focus for the first time. Billias admits candidly that Gerry came out of the Revolution "a very rich man" by mixing public and private business. There is no attempt to mask the fact that if Gerry the minister to France in the XYZ fiasco was right, it was only "for the wrong reasons." And finally, Billias openly concedes that the Marbleheader "was a failure" at the end of his life, as both governor and vice-president.

The final chapter is an excellent end to a good book. It places Gerry squarely in the framework of the American revolutionary era, commenting skillfully on biography as a means of depicting revolutionary ideology in general. No historian's work can be considered definitive anymore, but this biography need not be rewritten in this generation; no more can be asked of any biographer.

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ALFRED F. YOUNG, editor. *The American Revolution: Explorations in the History of American Radicalism*. DeKalb, Ill.: Northern Illinois University Press. 1976. Pp. xv, 481. Cloth \$15.00, paper \$5.00.

These thirteen new essays offer evidence of a rich and varied Left to the left of the Whig commonwealthmen, a diversity of radicalisms (and other discontents) which simply will not fit current one-dimensional paradigms. Dirk Hoerder sees "independence of action and a rudimentary class feeling" in Boston's "self-led" crowds. They went

"beyond . . . goals set by . . . Whig leadership" and clearly did not have "the mind of Whig pamphleteers." In the revolutionary South, Ronald Hoffman finds "a rather unpopular Whig elite" conceding much to the egalitarian demands of the "disaffected." In his examination of northern land riots, Edward Countryman finds inapplicable Pauline Maier's model, in which crowds act "as an extension of government." "Popular behavior is usually rooted in popular culture," he writes, and explores both in exciting fashion. His findings include popular "counter-governments" and diverse strands in popular ideology.

In Rhys Isaac's revolutionary Virginia, the "emergent counterculture" of the Methodists' and Baptists' "evangelical revolution" offered "a sharp challenge" to the gentry. The "oral culture of the people was . . . a form of rebellion against . . . the literary culture of the gentry." In the book's most methodologically adventurous essay, Isaac explores both cultures, concentrating on "body language" and "the dramaturgical structure of formal and informal gatherings." Accepting without apologetics the "tensions and ambiguities" in Thomas Paine's thought, Eric Foner restores his complexity and presents that thought with extraordinary clarity. And he is equally insightful about popular ideology, noting, for instance, that "'lower class' Philadelphia Presbyterians, who as early as 1765 exclaimed, 'no King but King Jesus,' also contributed something to the ideological origins of the American Revolution."

Many of the themes and conceptualizations underlying various essays are revealed more clearly if we move to very different turf: Francis Jennings' "The Indians' Revolution." In the traditional stereotype Indians, like the subjects of other essays, are "robot-like characters . . . respond[ing] reflexively to . . . commands from . . . real people." Jennings disagrees, and, like other essayists, restores his subjects' "agency," their contribution "by conscious efforts to the making of history" (E. P. Thompson). Jennings sees Indians initiating and whites responding. The change in perspective resonates with the self-activity of the Boston crowd, or the Virginia gentry trying to come to terms with popular oral culture. Similar themes are played out in Ira Berlin's "The Revolution in Black Life" in such black initiatives as: an offer to Dunmore by slaves *before* his better-known offer to them; slaves taking their freedom, and, with it, new names; slaves reconstructing families; slaves creating a new Afro-American culture.

Many of these essays redirect our attention to class and to economics. Marvin L. Michael Kay strongly rejects a sectional interpretation of the North Carolina Regulation, seeing instead a move-

ment of "class-conscious white farmers in the west" who "proposed democratic reforms to implement class rule." Joseph Ernst moves us closer to a stimulating economic interpretation of the Revolution. Criticizing studies which "divorce thought from social reality," Ernst singles out Bailyn's "too-simplistic consensus view" and the Morgans' "disembodied abstractions," (as well as William Appleman Williams' "tendency to discuss economic beliefs on the same high level of abstraction employed by Bailyn in his treatment of political beliefs.") As Ernst sees it, ideological concern with "economic realities . . . provided the essential impetus for . . . the 'revolutionary movement'." In a balanced account of the relationship between "Social Change and the Growth of Pre-revolutionary Urban Radicalism," Gary Nash sees Boston, New York, and Philadelphia "becoming centers of frustrated ambition, propertylessness, genuine distress for those in the lower strata." He criticizes "historians who concentrate on Whig ideology" for overlooking the "organic link between the circumstances of people's lives and their political thought and action."

Many of these essays do not satisfactorily establish the existence of distinctive ideologies (and Young makes no extravagant claims). This may be because distinctiveness was not there; or it may be because the methodologies employed do not adequately explore culture and consciousness. Many essays project consciousness from structures and events which certainly influence and reflect ideas but are not sufficient to explain them. George Rudé's magnificent tools for the study of crowds are useful, perhaps necessary, but certainly not sufficient to establish consciousness, or the lack of it. We will continue to look at crowds' targets of destruction. But this is at best a gross guide to what was on people's minds. Looking at broken windowpanes is not going to get us to class consciousness. We'd do better to ask the rioters directly what was troubling them. Are we sure that *conduct* is an adequate measure of attitude and consciousness? Behavior is not synonymous with attitude. Holding to the notion of a "history from the bottom up," I don't see how we can establish lower-class consciousness without looking directly into the minds of the lower classes.

Isaac, too, infers attitude from ambiguous conduct. Rioters' attitudes may not be legible in their conduct; similarly ritual, dance, and other public behaviors are heavily weighted with conventions which the investigator may confuse with their actual meaning to participants. Isaac's study of such behavior is ingenious and important, but we accept the myth of inarticulateness unless we maintain as our primary focus the words people used. Body language is less specific and more ambiguous

than verbal language. The conclusion that the "common folk" got the "dramatic symbolic significance" of resolutions read aloud, while not understanding their content, awaits confirmation from the "common folk." (Isaac sometimes approaches a McLuhanism which subordinates content to form.)

The stress on agency and self-activity, present in other essays, is absent in Joan Hoff Wilson's "The Illusion of Change: Women and the Revolution." Here women are objects, acted upon, people to whom history happens. Women's attitudes were "molded by . . . modernization trends"; "demography . . . determine[s] . . . destiny." Wilson repeatedly projects consciousness and attitudes from structural data. The "functional opportunities" available to women were "too limited to allow them to make the transition in attitudes necessary to insure high status performance." Women were not "being prepared to understand the political ramifications of the Revolution." (Even women's strengths come from elsewhere: "unusually strong father-daughter relationships produced a handful of exceptional women. . . .")

The essence of Wilson's definition of feminism is conscious antipatriarchalism, a standard by which "the most articulate of late eighteenth-century American women cannot be considered feminists." Fair enough. A reasonable next step might be to develop some definition which comes to terms with women's condition and consciousness at the time. Instead, Wilson dismisses modes of expression which have been found meaningful by other students of women's history. Women bread-rioters acted merely "out of an immediate concern for feeding themselves and their children and not for feminist reasons growing out of their age-old plight as women in a patriarchal society." The Great Awakening was an "opiate"; the Second Great Awakening was significant mainly for the "internalization of sexual inhibitions." Lacking "political astuteness," it was "literally . . . impossible for even the best educated females to understand the political intent or principles behind the inflated rhetoric of the revolutionary era." Wilson's insensitivity contrasts with a women's history which has greatly broadened our notions of power and politics.

In some of the essays, the attack on Bailyn's ideological approach has led to a generalized bias against ideological approaches. Hoerder, for instance, says—in words that might be read as unintentionally condescending—"rioters . . . did not simply act on . . . abstract principles or theories," but rather "when imperial policies and practices had a local impact and became issues in their daily lives." Similarly, Ernst perceives the reasonable contention that "values shaped behavior" in nar-

row fashion, and all but dismisses it. There is an unnecessarily exclusive flavor to Ernst's contention that "economic realities" provided "the essential impetus for . . . the 'revolutionary movement'" and that "self-interest, not constitutional principle, brought . . . merchants and mechanics together." Is it true that "there is too much analysis of the Revolution's superstructure—ideas, principles, politics, and culture—and too little of its substructure—political economy"? We need *better* studies of elite thought, not *fewer*. In addition, Ernst's polarization unintentionally ignores the study of *popular* ideas, principles and culture—of which there has been very little. Until we actually carry through such studies, our discussion of "mentality" will rest on faulty foundations. There are better ways to study ideology than those which Ernst justly criticizes. Calling it off is a case of throwing out the baby with the Bailyn.

Among those frequently cited in this book are Thompson, Hobsbawm, Rudé, Hill, and Heimert. As Young puts it in his excellent "Afterword," we have "tantalizing glimpses" of something like what Thompson calls the "moral economy of the poor." Those glimpses, although partially unsatisfying, are also enormously promising. If they do not allow us, as Young readily acknowledges, "to claim a distinct ideology among those below," they will certainly no longer allow scholars "to continue on the bland assumption of a consensus behind a single patriot ideology descended from a single English tradition." In assembling and actively editing these extraordinary and challenging "explorations," Young offers "findings on a search that has only begun." Taking this book as their reference point, scholars will be at work for many years, trying to come to terms with the bright new questions raised by Young and his essayists.

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J. LEITCH WRIGHT, JR. *Florida in the American Revolution*. Gainesville: University of Florida Press. 1975. Pp. xvi, 194. \$7.50.

Amid the flurry of books and articles honoring America's Bicentennial, at least one fact should cause historians to pause before they refer to the "thirteen English Colonies." There were, on the mainland of North America, not thirteen, but seventeen colonies. We usually dismiss Nova Scotia and Quebec since they never became part of the United States, but we also frequently omit East Florida, with its capital at St. Augustine, and West Florida, with population centers at Pensacola, Mobile, Natchez, and Baton Rouge.

In this excellent survey J. Leitch Wright sets the

record straight. He has used a great variety of primary source materials—something which has been greatly needed—and unlike most historians of the British colonial experience, he is familiar with Spanish archival records. The bibliographical essay is especially valuable for scholars seeking additional data. Wright has previously studied the fascinating career of William Augustus Bowles in Florida, and his *Anglo-Spanish Rivalry in North America* (1971) gave readers a glimpse of what was to come in this current volume.

Spain was not a direct ally of the infant United States during the American Revolution, but the Family Compact among the Bourbon cousins of Spain and France did bring Spanish aid which indirectly resulted in considerable benefit, not only for the George Rogers Clark expeditions, but for diverting considerable British armed forces in West Florida. It was the wartime campaigns of Louisiana governor Bernardo de Gálvez which swept the British from their Gulf of Mexico provinces and, indirectly, gave rise to the later boundary dispute with the United States over the true location of the Louisiana Purchase in the East.

Wright separates East and West Florida in his discussion, as well he should, and he points to frontier relations during the conflict as a harbinger of postwar difficulties. Some of the maps are too light to read, and the illustrations could be better. It is in the smoothly written text, however, that the author makes his best contribution, and this reviewer readily agrees that it is the best book published in 1975 on Florida. For it Wright received the annual Rembert W. Patrick Memorial Book Award. The volume should occupy a place on any shelf of American Revolutionary history.

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MERRILL JENSEN, editor. *The Documentary History of the Ratification of the Constitution*. Volume 1, *Constitutional Documents and Records, 1776-1787*; volume 2, *Ratification of the Constitution by the States: Pennsylvania*. Madison: State Historical Society of Wisconsin. 1976. Pp. 391; 779, with microfiche supplement. \$20.00; \$27.50.

These volumes inaugurate publication of a major resource for American historians, one that replaces Jonathan Elliot's *Debates in the Several State Conventions on the Adoption of the Federal Constitution*. It is evident that Merrill Jensen's work, which is to consist of fifteen volumes, plus lengthy supplements in microform, will be at once much more scholarly and much more comprehensive than Elliot's. The first volume of the new work provides a background of national, or at least, interstate action for the next ten volumes, which are to deal

with ratification of the Constitution by the states. The final four volumes of the set are to be devoted to public and private commentaries that circulated through many of the states at the time of the ratification debate.

The initial volume might at first glance seem overly inclusive, but cross-references in the second volume prove the pertinence of these preliminary documents. The first of them is the Declaration of Independence, followed by the Articles of Confederation and related materials, such as state acts ratifying the Articles, proposed amendments, and the famous ordinances for the survey and government of the western territories. The last half of this volume consists of records pertaining to the Federal Constitution, including proceedings of the Annapolis Convention and plans proposed at Philadelphia in 1787. An interesting concluding section deals with the debate in Congress before that body forwarded the Constitution to the states. Each section is preceded by an introduction, and headnotes and endnotes accompany almost every document or set of documents. Some repetition exists (compare, for instance, pages 178 and 192 or pages 239 and 298), but probably this is justified by the likelihood that students will be consulting individual items rather than reading through this book.

The second volume is devoted wholly to Pennsylvania, and a fat volume it is, though not larger than the 1888 volume on the same subject edited by John Bach McMaster and Frederick D. Stone and entitled *Pennsylvania and the Federal Constitution, 1787-1788*. Jensen, however, will include many more documents than McMaster and Stone because he relegates to his final volumes of commentaries many Pennsylvania items which, because they circulated widely, seem best viewed as part of a national debate. Moreover, Jensen has provided an additional and unobtrusive resource, a microfiche supplement to the Pennsylvania volume consisting of more than 2,700 pages. This is a repository for items (largely newspaper articles) that are either repetitious or peripheral to the main debate but which amplify the arguments presented for and against ratification.

The struggle over ratification in Pennsylvania has never been so fully documented before, nor, and this is hardly a lesser compliment, so clearly delineated. Jensen chose to give precedence to Pennsylvania because it was the first state to call a convention, though only the second state to ratify. In general he plans to turn from one state to another in the order in which state conventions met.

Besides being attractively printed and bound, these volumes contain several thoughtful touches: a ratification chronology, calendars for the years 1787-1790, a map of Pennsylvania showing county lines in 1787, and a "biographical gazetteer," identifying the chief political figures in Pennsylvania.

All involved in this work deserve congratulations; no student of the period should neglect this splendid scholarly achievement.

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BOYNTON MERRILL, JR. *Jefferson's Nephews: A Frontier Tragedy*. Princeton: Princeton University Press. 1976. Pp. xv. 462. \$16.50.

One of the most appalling atrocities of American slavery was the mutilation-murder of a seventeen-year-old male house slave in Livingston, Kentucky, in 1811 by two nephews of Thomas Jefferson. Celebrated in abolitionist literature, and the subject in 1953 of Robert Penn Warren's dialogue in verse, *Brother to Dragons*, this grisly story has finally been researched with sensitivity and in scholarly detail. As Boynton Merrill, Jr. reconstructs the event, Lilburne Lewis, 36, recently widowed father of five, has remarried, and his wife, Letitia, is expecting her first child. Debt-ridden and drinking heavily, Lewis becomes enraged when the slave, George, who has recently tried to run away, breaks a valuable pitcher which had belonged to Lewis' mother. Aided by his younger brother, Isham, Lewis ties the slave to a meat chopping block, hacks him to pieces before the other horrified slaves, and then tries to burn the remains in the kitchen fireplace.

The incineration is interrupted when an earthquake destroys the chimney, Lewis and his slaves rebuild the chimney, hiding the charred remains behind the stones. Succeeding tremors again destroy the chimney; a dog carries off the skull, and the secret can no longer be kept. Even though the slaves are the only witnesses and cannot testify against a white man, the two brothers are indicted for murder. They plan a suicide pact, but only Lilburne kills himself, dying on the grave of his first wife, Elizabeth Woodson Lewis, and leaving a cryptic suicide note which says, "I have fallen victim to my beloved but cruel Letitia. I die in the hope of being united to my other wife in heaven." Isham escapes before his trial and disappears.

Merrill sifts fact from legend and speculates on the motivations for the crime, agreeing in part with Jefferson, who never commented directly on the horror, but who did write later that the Lewises were given to "hypochondriacal affections," and "sensible depressions of mind." Merrill emphasizes also Lilburne Lewis' impending bankruptcy, his alcoholism, and his unhappiness with his new wife, who fled from home with her infant son some weeks after the murder, fearing for her own life. Every possible motivation for the horror is explored save one, which might further explain the incredible brutality; the drunken, depressed Lewis may have fantasized that the young

slave had taken liberties with his "cold, proud and scornful young wife."

The murder story explodes dramatically in the last third of Merrill's book. Earlier pages are devoted to a somewhat tedious account of the slowly declining fortunes of Colonel Charles Lilburne Lewis, who had married Jefferson's sister Lucy and had moved to Kentucky with his sons in the futile hope of regaining his wealth. Jefferson scholars will read with interest Merrill's meticulous reconstruction of the heretofore largely unknown financial involvement of Jefferson with members of Colonel Lewis' family and other relatives. There is a good deal of local Kentucky history, some of it irrelevant, some fascinating (like the chapter on the great earthquake). Of special value are the verbatim reproductions of the earliest documents concerning the murder, especially the account by the local pastor, Reverend William Dickey, who described Lewis as a slaveholder who "drove constantly, fed sparingly and lashed severely." Dickey was so horrified by the murder that he moved to Ohio and became an ardent abolitionist.

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JOHN A. ANDREW III. *Rebuilding the Christian Commonwealth: New England Congregationalists and Foreign Missions, 1800-1830*. Lexington: University Press of Kentucky. 1976. Pp. 232. \$14.50.

By the beginning of the nineteenth century, the standing order of New England was in retreat and disarray. The Unitarian schism had greatly weakened traditional Congregationalism's credibility as a state-supported church, and disestablishment was just around the corner. Ministers were moving ever more frequently, seeking the better-paying parishes which provided greater job security. The society in which Congregationalism operated was changing equally rapidly. New England was industrializing and urbanizing at the same time that newer and more productive agricultural regions were threatening what remained of its rural districts. Not surprisingly, the clerical leadership of Congregationalism sought ways to respond to the new conditions.

According to John A. Andrew III, the clergy turned from internal revivalism—the traditional Puritan response to declension—to a foreign missionary activity they hoped would transcend the many divisions and unite all parishioners in benevolence. This book devotes about equal attention to explaining Congregationalism's foreign mission thrust in terms of the denomination's social decline and to detailing the first missions to the Sandwich Islands. Despite the author's stated attempt to present the missionaries in a more fa-

avorable light than previous studies have managed, this work has the curious effect of making them see even less sympathetic figures. An unremitting concentration upon the social background of the missions and the missionaries produces one-dimensional characters discussed in a manner reminiscent of biographical entries in an encyclopedia. The facts are clearly presented, but Andrew never probes beneath them to produce flesh-and-blood people.

One notes with considerable dismay the selling price of this relatively brief and unimaginatively packaged volume, which will hardly help enhance its circulation. The economics of academic publishing seem increasingly to become self-defeating.

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ROBERT J. LOEWENBERG. *Equality on the Oregon Frontier; Jason Lee and the Methodist Mission, 1834-43*. Seattle: University of Washington Press. 1976. Pp. xi, 287. \$17.50.

Robert J. Loewenberg has significantly updated the history of Jason Lee and the Methodist mission to the Indians of Oregon's Willamette River Valley. He discovered a cache of unknown Lee manuscripts and fashioned a coherent view of the mission and its place within the antebellum evangelical Protestant movement. Loewenberg's most lasting contribution may be his demonstration of the value of local history as a laboratory in which to study broad historical trends.

Loewenberg presents a welcome refutation of the idea that the missionary impulse issued from a desire for "social control." He points out that proponents of the social control theory stress "progress, reform, change, and rationalism," and that Jacksonian America was instead "deeply and unashamedly consensual." Methodist evangelicals assumed that "Christian harmony and democracy [were] two sides of the same coin." Through missionaries like Jason Lee, they hoped to create a world of harmonious communities.

Desire for harmony was behind Lee's involvement in Oregon politics. Loewenberg shows that Lee used economic leverage in trying to create a harmonious community. According to Loewenberg, local government in Oregon did not begin with the settlement of entrepreneur Ewing Young's estate in 1841, but rather with the cooperation of Lee and the Hudson's Bay Company to quash Young's plans to build a distillery in 1837. Lee, of course, supported temperance; the Hudson's Bay Company countenanced no challenge to its role as sole area supplier of goods and services.

Past opinion has held that Lee's overwhelming involvement in the area's political affairs eventually forced the Methodist Board of Managers to

dismantle the mission. Lee, Loewenberg feels, adhered to a pure evangelicalism, endeavoring to keep Christianization and civilization separate by establishing mission stations distant from the home base at Willamette. He became a scapegoat, personifying the ideal missionary that the Methodist Board was submerging in its growing emphasis on secular settlements as vehicles for Christianization. Loewenberg claims that the imbroglio arose from the Board of Managers' increasing acceptance of the policy that Christianization went hand in hand with civilization.

By insisting that the Methodist missionaries originally separated Christianity and civilization, Loewenberg misconstrues their millennial aspirations. Evangelical Protestants mounted missionary campaigns to transform the American wilderness into a middle landscape peopled by pious farmers. In 1825 the American Board of Commissioners for Foreign Missions stopped building large, self-sufficient Christian communities such as Lee established in Oregon ten years later. Instead, they developed interdependent, scattered mission sites within Indian nations. Smaller stations provided Indians with examples of husbandry and the benefits of Christian habits. The board's Oregon mission, constructed on this model, ran concurrently with the Methodist mission. In the early 1840s it appeared more successful in teaching the Indians the ways of Christian husbandry than the Methodists' settlements. Rather than a misunderstanding between Lee and the managers regarding Christianizing versus civilizing, Lee's dismissal represented the Methodists' realization that large establishments were impractical. Lee was neither capable of managing a large mission nor willing to reduce its size. He had to be removed.

The history of missionary enterprises in antebellum America is an extended debate on how to impose on the world a vision of Christian republicanism, not, as Loewenberg implies, the declension of evangelicalism into a more base secular form.

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STANLEY M. GURALNIK. *Science and the Ante-Bellum American College*. (Memoir of the American Philosophical Society, number 109.) Philadelphia: the Society. 1975. Pp. xiv, 227

The history of American science must include higher education, just as any assessment of the history of higher education must consider the place of science. Stanley Guralnik has joined these two themes in a neatly delimited and ably executed monograph on science in American private colleges in the period 1820 to 1860. For his sources he has plumbed the archives of the colleges, gath-

ering ample evidence for an interpretation which significantly alters the usual picture of tyranny by the classics and stubborn neglect of the sciences.

After presenting a picture of science in the eighteenth-century college which is a good deal less enthusiastic than the standard account by I. B. Cohen, Guralnick is in a position to characterize the 1820s as a decade of revolution. George Ticknor's well-known reforms for Harvard led to changes in the science offerings, and a Yale report assessing the teaching of the classic languages advocated making place for new sciences as they appeared without transforming the college into a university. It was this legacy of incorporating science into the college curriculum that the 1820s gave to the American colleges all the way to the Civil War.

By the 1850s so much science had been added that, according to Guralnick, "students who attended the traditional colleges . . . were obliged to devote more time to science than they ever had before or would again." Separate chapters trace the varying emphasis on mathematics, physics, and astronomy, leaving the distinct impression that Guralnick considers the physics-centered twentieth-century model as definitive. "Chemistry and its College Derivatives" is a chapter heading which includes chemistry, geology, and all of natural history. The evidence for this weighting of the various disciplines is far from persuasive. Especially in the case of Harvard, a whole tradition of natural history, already elaborately explored in secondary works, is simply ignored. An insightful chapter on "Utility and the Science Courses" ties the colleges to larger social and economic forces and shows that the experimentation of the 1850s prepared the way for the land-grant colleges.

In such a short monograph, to devote fifty-one pages to an alphabetized biographical dictionary of science professors is a questionable use of resources, especially since the list adds little to Guralnick's major contribution—a reinterpretation which places science in the antebellum college in a credible context.

A. HUNTER DUPREE
Brown University

ALAN L. OLMSTEAD. *New York City Mutual Savings Banks, 1819–1861*. Chapel Hill: University of North Carolina Press. 1976. Pp. xiv, 236. \$15.95.

This is a useful study of the beginnings of savings banks in New York City. Alan L. Olmstead briefly describes some embryonic European precursors and the intellectual origins in the reformist ideas of the Utilitarians and their American counterparts. Philanthropic purposes were modestly fulfilled in the form of savings havens for some of the poor; but the vaults swelled with middle- and upper-

class deposits, to the initial surprise and annoyance of the founders. State legal constraints funneled the money via governments into developmental projects basic to local and regional growth, notably the Erie Canal and the Croton Reservoir, and the rescue of fire insurance companies and their clienteles after the Great Fire of 1835. The developmental effect was the most important: the new institutions materially eased major infrastructural investments, and at lower rates of interest than otherwise would have been possible. In time, as available investment opportunities were exceeded by savings inflows, legal restrictions were eased; although at first this process too was made to serve New York interests by developing in similar fashion the tributary Ohio canals. Ultimately, deposits were also lodged in commercial banks and put out in call loans. The author makes comparisons to practices elsewhere in the country as the literature permits. In places he confirms the concordance of his historical conclusions to economic theory, and translates them into mathematical formulae and graphs for the benefit of those economists who do not comfortably read English.

There is reason to regret that Olmstead did not generalize more, and he sometimes fails to establish the context of significant material. Stimulating ideas appear at the beginning of chapter one, only to disappear as suddenly, unexploited. The ruling attitudes and policies of the early stages could profitably have been expanded upon. The discussion of interlocking directorates among savings and commercial banks might well have been pushed farther, and private bankers and brokers are a neglected factor. Some of the most fascinating and significant pages (35–47) dovetail closely with other evidence on cohesion and fragmentation in the structure of antebellum banking; but the discussion is vitiated in the settings of "mutual dividend policy" and "mutual surplus accounts," and even in the brief section on "causes." In short, the knowledgeable reader will perceive much that the journeyman may miss. Nevertheless this valuable and suggestive book is likely to remain the standard study of early American savings institutions.

HENRY COHEN
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Chicago*

JAMES C. CURTIS. *Andrew Jackson and the Search for Vindication*. (Library of American Biography.) Boston: Little, Brown and Company. 1976. Pp. xi, 194. \$7.50.

Psychoanalysis has become a familiar, if not always a fruitful, approach to history. The present instance is no more nor less successful than the

genre as a whole. As interpretive history, however, it transcends its premises. One does not have to seek motivation in Jackson's early relationship to his mother in order to appreciate the validity of James C. Curtis' reappraisal of his flamboyant career. The facts as we know them are quite compatible with overweening ambition and a ruthless lust for power. The aggressive, brawling youth of the North Carolina years became in Tennessee a young man on the make, bent upon allying himself with the ruling class and making a fortune. The ownership of slaves, the dueling, the land speculation, the Indian fighting, the flirtations with Spain and with Aaron Burr all fit into the same pattern. So do Jackson's undeviating loyalty to the Blount faction, his carefully cultivated alliance with the Donelsons, and each successive office held on the way up the state political ladder. Even the office of major general of militia was in his eyes a political rather than a military office, whose value lay in the intimacy it offered with the leading families who would be represented by his subordinate officers. His military career, when war came, was compounded of ineptitude, foolhardiness, blunders that somehow turned to his advantage, stubbornness, and luck. Yet the Battle of New Orleans made him the outstanding hero of the conflict, and perhaps the most popular man in America.

The Jackson who emerges from these pages is devious, secretive, not above double-crossing a friend (though he would seek the ruin of any friend who renounced him), an intriguer, and often enough an ignoramus in government affairs; yet he became in his own time a national leader whose every move was right. He behaved less like a man seeking vindication for a traumatic past than like a new Messiah, sure that he could do no wrong and prepared to eliminate anyone who said he could. Every show of opposition was the work of "enemies" who appeared conveniently when needed or could be imprecisely articulated out of an unclear background. Yet his personal vendettas, such as those with Calhoun, Biddle, and General Scott, helped to mold the presidency and to give direction to the nation. He seems less often seeking for vindication than grasping for power. The Jackson story, told from a point of view not common among the present generation of historians, provides a worthwhile antidote to the familiar myth, even without the author's not always successful effort to assign motives.

CHARLES M. WILTSE
Dartmouth College

ODIE B. FAULK. *The U. S. Camel Corps: An Army Experiment*. New York: Oxford University Press. 1976. Pp. ix, 213. \$9.75.

The exotic appeal of the U.S. Army's camel experiment of the 1850s has long attracted historians, mainly of the local color school. While Odie B. Faulk has not completely escaped this genre, his study of the "Camel Corps" (really a misnomer as the animals were never organized as a separate unit) draws upon official army records in the National Archives and is the most thorough and scholarly treatment of the subject yet to appear. Faulk traces the experiment to the military occupation of the Southwest after the Mexican War, which raised unprecedented problems of supply and operations over vast arid and semi-arid regions. Through the lobbying of quartermaster officers who won Secretary of War Jefferson Davis to their cause, the government brought about seventy-five camels and several handlers from the Middle East to Texas. The animals performed well in a number of trials, surpassing mules and horses in strength and endurance. However, army personnel found them difficult to handle, their odor frightened other animals, and the project as a whole suffered from a lack of focus: quartermaster officers urged breeding a large herd for both civilian and military transportation while the War Department favored a narrowly military function. The camel experiment languished during the Civil War and gradually faded thereafter, as the completion of the transcontinental railroads eased the army's transportation problems.

Faulk's account is stronger on description than analysis. He includes a vast amount of detail, much of which seems of questionable relevance and tends to obscure his principal findings. Does the reader really benefit from two pages on the payroll of the camel drivers (pp. 81-82) or the itinerary of the Arabs' return voyage to Egypt (pp. 151-52)? More seriously, Faulk might have broadened the significance of his study by relating the camel experiment to the general evolution of frontier military policy, or, in the tradition of Walter Prescott Webb, by viewing it as a part of the American people's multi-faceted response to the semi-arid West. Nevertheless, his book is informative, thoroughly researched, includes many excellent illustrations, and should stand as the definitive work on a minor but colorful episode of western history.

WILLIAM B. SKELTON
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Stevens Point*

HERBERT G. GUTMAN. *The Black Family in Slavery and Freedom, 1750-1925*. New York: Pantheon Books. 1976. Pp. xxviii, 664. \$15.95.

Herbert G. Gutman's book has received much publicity in the news media as well as in long

reviews, both laudatory and critical. Challenging traditional theories of black family disorganization and "pathology," Gutman contends that even centuries of slavery and injustice failed to weaken the remarkable "adaptive capacities" of Afro-Americans. At least until the 1920s, he argues, blacks managed to preserve cultural traditions, family continuities, and the "double-headed kin related household."

The first eight of Gutman's ten long chapters deal with the black family in slavery, and his central evidence consists of plantation records, a few surviving county census schedules, and Freedmen's Bureau records, especially of validated marriages. Whatever the faults of Gutman's study, this information is invaluable—in the place of a few observers' impressions, one is now confronted with *some* information about the lives of tens of thousands of largely illiterate and unskilled Americans, the ancestors of living Americans. This evidence, often interpreted with skill and imagination, dramatically reinforces the key theme of the best recent works on American Negro slavery: that the worst barbarities of human bondage could coexist with a kind of accommodation between black and white cultures. The controversial point is whether the relative autonomy and perseverance of black family life was a wholly independent force, as Gutman maintains, immune from white-determined rules of the game, rules pragmatically chosen to ensure the stability of the slave regime.

Gutman is not always sensitive to the limitations and ambiguity of his evidence. To give only two examples, he almost wholly ignores the widespread practice of "hiring" slaves out for a year or more in the most lucrative labor market, a practice detrimental to family stability but seldom recorded except in probate proceedings where possible claims could arise involving the transmission of title. Second, in arguing for the relative stability and durability of slave marriages, Gutman places great weight on the demography of four Virginia counties in 1865 and 1866. Yet in three of the counties over half the adult black males were over the age of forty (in York County, 57 percent). In 1860 the national figure for black males of the same age group was 34 percent. Gutman makes no allowance for this disproportionate age and sex structure. Nor does he emphasize that the fourth county, Montgomery, is west of Roanoke, in the mountains, or that it had a low proportion of blacks and a relatively high proportion of black single-parent households. It is significant that what Gutman terms "the most important single piece of historical evidence in the book"—the dust jacket photograph of "Five generations on Smith's plantation, Beaufort, South Carolina"—does not portray a single young adult male.

The crucial problem with Gutman's book is his reification of "culture." His previous essays presented a refreshing challenge to the notion of inert, victimized peoples (mainly immigrants) being shaped and "modernized" by Anglo-American elites. No doubt historians have often adopted a simplistic model of acculturation, and have accepted the self-congratulatory beliefs of "victors" who at best won only stalemates. Yet Gutman has now overreacted to the point of denying the realities of power, not only physical power (of a sort immigrants seldom felt), but the subtle cultural and psychological influences that emanate from superior power when it is held with conviction for any length of time. In this respect, his book is profoundly naive and perhaps reflects the rigid, idealistic, and romantic dichotomies of the 1960s, when, for a moment, power did not seem to matter. Much as Gutman wants to deplore the injustices of slavery, he seems in the end to suggest that it did not really matter—nor did white power and cultural hegemony from Reconstruction to the Great Depression. While Gutman rightly condemns the historians who ignore the dynamic flexibility of the historical process, he tends to fall into a worse trap of separating "culture" from the social structures of power. He thus blinds himself to the prerequisite for successful and enduring power: the ability to define priorities and to accept the "cultural adaptations" of underlings when they pose no threat to vital interests. "Adaptive culture" is the central theme of Gutman's book, but it becomes a meaningless cliché when deprived of social context—that is, the knowledge of who is adapting what to which, and why.

DAVID BRION DAVIS
Yale University

PAUL A. DAVID *et al.* *Reckoning with Slavery: A Critical Study in the Quantitative History of American Negro Slavery.* Introduction by KENNETH M. STAMPP. New York: Oxford University Press, 1976. Pp. xvi, 398, Cloth \$15.00, paper \$4.50.

The contributors to this elaborate critique of *Time on the Cross* by Robert W. Fogel and Stanley L. Engerman are extremely well qualified for their task, and no short review can do justice to the variety of criticisms that they advance.

In an introductory "Humanistic Perspective" Kenneth Stampp accuses Fogel and Engerman of destroying straw men, of ignoring or distorting earlier findings, of disregarding the "irrationality, friction and conflict" of slavery and of using patronizing "antiracist rhetoric." Paul David and Peter Temin suggest that Fogel and Engerman inferred the "motivation of individuals from evidence about the outcome of a market process,"

ignored moral dimensions, greatly underestimated slavery's "rate of exploitation," and used faulty methods in showing that southern agriculture was more efficient than northern farming. Herbert Gutman and Richard Sutch argue that Fogel and Engerman seriously misjudged the incidence of whippings, rewards, and opportunities to acquire skills and exercise leadership on plantations, as well as the quality of work performed there. Slave families, they maintain, were dispersed and female slaves subjected to sexual abuse much more frequently than Fogel and Engerman estimate. Admitting that the gross caloric content of slave diets was high, Sutch suggests that their general composition and mineral and vitamin content were unsatisfactory. Gavin Wright argues that the mid-nineteenth-century cotton boom explains the productivity of southern agriculture and that the exceptional census crop year, 1859, skewed Fogel's and Engerman's results. Nor could the South, in Wright's estimation, have "sustained growth in the modern era."

The authors of this volume pronounce "the structure" of *Time on the Cross* "distinctly unsafe for continued occupancy" (p. 343), but the bare-knuckled abandon and technical virtuosity of cliometric argument often appear more devastating to the outsider than to the combatants. Fogel's and Engerman's models are susceptible to challenge, and they used conventional sources less intensively than desirable. They oversimplified the literature of slavery; perhaps even made mistakes in computation; and they were overly audacious in interpreting their findings. But basic elements in their analysis still stand, and their enterprise has initiated an amazing burst of creative scholarly energy. Commendably, they have encouraged criticism of their work and surrendered their data sets for secondary analysis more rapidly than most researchers. Their use of neglected types of sources is encouraging the more intensive utilization of such materials. Finally, they experimented boldly in developing a publication format designed to make their findings intelligible to the general reader.

The contributors offer *Reckoning* as a "scientific contribution . . . to the history of antebellum southern society" and "not a polemic." Hits they make, but there are also strikeouts and dubious "calls." Sometimes quantitative analysis contributes to scholarship by substantiating one of several hypotheses already advanced; to show that a point has already been made does not negate the quantifier's contribution as Stampf seems to imply. Despite the shrewd criticism of Temin and David, reasoning backward from outcomes has materially advanced scientific knowledge, and various assumptions in their critique of Fogel's and Enger-

man's comparative efficiency index are debatable. Sutch and Gutman display a disconcerting tendency to torture texts and press contrary evidence into service without critical analysis; they reject the contention that slaves internalized the Protestant work ethic without reference to the work behavior of appropriate control groups. And indeed, much of this book is devoted merely to suggesting that alternative explanations are possible. If *Time on the Cross* abounds with value judgments (and it does), they are to be found in *Reckoning* as well.

This volume is not the last word in the controversy surrounding *Time on the Cross*, but it makes an extremely important contribution to the debate and to the development of a more sophisticated understanding of American slavery.

ALLAN G. BOGUE
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Madison

CLAUDIA DALE GOLDIN. *Urban Slavery in the American South, 1820-1860: A Quantitative History*. Chicago: University of Chicago Press. 1976. Pp. xv, 168. \$12.95.

Historians have interpreted the decline of the urban slave population during the 1850s as an indication of the institution's weakness in the southern city. Claudia D. Goldin, a protégé of Robert W. Fogel, asserts, however, that the exodus of urban slaves reflected an *increased* demand for their labor in the cities rather than a decline. Goldin depicts urban slavery as a vigorous, flexible institution that allowed the slave considerable latitude in the choice of employment and residence. Some urban slaves attained relatively high skill levels. These conclusions, presented in jargon-free prose, should not be surprising to scholars familiar with the works of Robert S. Starobin and Charles B. Dew.

Goldin attempts to reconcile this positive view with the fact of population decline during the 1850s. First, she demonstrates that the demographic changes of the decade were part of a larger trend of population cycles dating from 1820. Second, the author constructs an econometric model to measure demand, introducing the concept of elasticity of demand—"the responsiveness of quantity to changes in price"—to explain the seeming contradiction between a popular labor system and a decline in its force. When slave prices rose during the 1830s and 1850s, urban slaveowners found it profitable to sell their slaves to rural areas. How does this relate to urban demand? "The crucial aspect of urban slavery. . .," Goldin responds, "was the greater ability of urban slaveowners to substitute other inputs for slave labor in times of rising slave prices." That is, white women, free

blacks, and immigrants satiated the rising urban labor demand. These labor forces were not available in sufficient quantities in rural areas, nor could they replicate the economies of scale that slavery brought to plantation agriculture.

Some of the methodological problems which plagued *Time on the Cross* also trouble Goldin's efforts. The limited number of observations (four), the paucity of reliable data (the use of slave prices in Richmond, for example, as a proxy for slave prices in Louisville), and the conclusions drawn from statistical procedures using such data are weak supports for an otherwise convincing argument. Finally, Goldin's emphasis on the substitutability of slave labor in cities begs for a discussion of the extent and nature of alternative work forces in southern cities. Why, for example, did Richmond, where immigrants comprised one-quarter of the white adult population, exhibit such a low elasticity of demand (i.e., low substitutability)? Could a city's economic base (Richmond was one of the South's few industrialized cities) be a more important determinant of demand than the presence of substitute labor forms? The retention of skilled slaves in times of declining slave population suggests this view.

Despite its methodological flaws, the book is an important contribution to our knowledge of slavery and cities. It raises necessary questions about life and labor in southern cities that demand further exploration.

DAVID R. GOLDFIELD
Virginia Polytechnic Institute
and State University

MARION BRUNSON LUCAS. *Sherman and the Burning of Columbia*. Foreword by BELL I. WILEY. College Station: Texas A & M University Press. 1976. Pp. 188. \$10.95.

On February 17, 1865 troops commanded by William T. Sherman occupied Columbia, South Carolina. Dawn on February 18th found much of the city a smoldering ruin. Confederates and latter-day professional Southerners saw a simple cause-and-effect relationship between these events and listed the burning of Columbia as but one item in the long catalogue of atrocities committed by Sherman in his crusade against the South in general and South Carolina in particular.

But was the federal commander responsible? This is the question that Marion Brunson Lucas attempts to answer. Lucas presents convincing arguments that Sherman was innocent of the arson charge. Rather, confused and inept Confederates actually began the fires (to burn cotton and keep it out of Sherman's hands), and high winds spread the blaze and nullified efforts to bring it under

control before one third of Columbia's buildings were destroyed. Drunken Yankees may have fired some buildings and looted others, but such incidents were rare. Violence against citizens was almost nonexistent, there were no reported rapes, and the only known deaths were those of two Northern soldiers killed by Federal troops sent into the city to establish order. These opinions are not new. James Ford Rhodes arrived at much the same conclusions in "Who Burned Columbia?" *AHR*, 7 (1902)—an item absent from Lucas' bibliography—and elaborated upon them in his *History of the United States* (1904), 5:90-98.

Some caveats are appropriate. Lucas' major sources are reports of committees or commissions, and the testimony of Northerners before those bodies will not be accepted by people who want to hold Sherman responsible. Further investigation of contemporary writings of Federal soldiers would have been helpful (fewer than ten items in the bibliography are Unionist letters and diaries). Parts of the work are footnoted to distraction. Appendix A (Inventory of Ordnance Stores), taken from the *Official Records*, seems unnecessary. Omission of the first two words from the title would more accurately reflect the book's contents. However, the question of who burned Columbia has been settled for all but the most unreconstructed rebels.

RICHARD M. MCMURRY
Valdosta State College

C. PETER RIPLEY. *Slaves and Freedmen in Civil War Louisiana*. Baton Rouge: Louisiana State University Press. 1976. Pp. xiv, 237. \$10.95.

The title of this volume promises a study of slaves and freedmen in Civil War Louisiana. The title is misleading. C. Peter Ripley is principally concerned with federal policies and programs affecting blacks and with the whites who administered them. Only two chapters directly consider the "expectations" of blacks to which Ripley refers in his conclusion. In addition, Ripley is almost exclusively concerned with federal policies in the military Department of the Gulf. Northern Louisiana, from the Red River to the Arkansas border, is barely mentioned, and no attention is paid to the distinctive character of freedmen programs in the Louisiana parishes under federal control in the Vicksburg and Natchez districts.

In his study of federal policies toward blacks in the Department of the Gulf, moreover, Ripley has difficulty breaking new ground. Despite his narrative skill and his careful search for sources, scholars familiar with recent studies of Reconstruction in Louisiana—notably J. Peyton McCrary's 1972 Princeton dissertation treating the failure of Lin-

coln's reconstruction plans for Louisiana and John W. Blassingame's *Black New Orleans* (1973)—will find much that is familiar in Ripley's sources and thesis. Ripley acknowledges the importance of these and other works in his bibliographic essay, but in the notes to the text, the author rarely mentions that scholars have recently studied most of the manuscripts he cites and developed substantially the same conclusions.

Closer attention to these secondary works, in fact, might have prevented errors and omissions in Ripley's work. Francis Lieber (whose "Memoir on the Military Uses of Colored Persons" set the tone for federal policy toward southern blacks) becomes Francis Lubber in Ripley's study. Ripley's two-page treatment of the Ford Jackson mutiny of black troops fails to mention Fred Harvey Harrington's detailed article on the subject although Harrington's biography of General Banks receives Ripley's praise.

The familiarity of much of Ripley's story unfortunately obscures the importance of his two chapters on Confederate slavery and the family. Ripley's discussion of Confederate slavery, which points to the "disruptive influence of the war and Confederate policies on the stability of slavery," together with his attention to the plight of the black family under Rebel and Yankee control, offers a substantial glimpse of the reality blacks experienced. The relationship between master and slave, as Ripley explains, was weakened first by Confederate mobilization and later by federal occupation. The master-slave relationship often collapsed and always changed dramatically as slaves acted on their own to secure freedom, to survive, and to reknit the fabric of their family and community life.

It is because Ripley considers these themes that readers will find his work useful and, perhaps, important.

LOUIS S. GERTEIS
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St. Louis*

CHARLES VINCENT. *Black Legislators in Louisiana during Reconstruction*. Baton Rouge: Louisiana State University Press. 1976. Pp. xv, 262. \$15.00.

Although the era of southern history following the Civil War has been known as "Black Reconstruction," historians know little of the black politicians who played so important a role in it. With the exception of a few of the most eminent leaders—P. B. S. Pinchback, Robert Smalls, Tunis G. Campbell, and a handful of others—they remain faceless unknowns. With this study Charles Vincent attempts to rectify the omission for Louisiana. Studying tax rolls, city directories, pension files,

and records of the Freedman's Savings Bank, parishes, and military, Vincent has uncovered basic information about black leaders during the Civil War and black legislators from 1868 to 1876. To this information he adds a general record of black activities in the state legislature.

Unfortunately, Vincent does not integrate his information well into the political environment of Reconstruction Louisiana; he only sketchily outlines the incredible gyrations of Louisiana politics from 1868 to 1876, offering too little to enable readers who are not already familiar with the tangled tale to follow it. Without a political framework within which to act, Vincent's subjects do not come alive. The reader is told what committees black legislators sat on and what bills they introduced, but he is not informed of what role they played in the chaotic factionalism that lay at the heart of Louisiana Reconstruction, or how they were chosen to represent their constituencies, or what role they played in the black community. Also, Vincent offers no composite description of black legislators until his final seven pages, and that description is far too brief. An additional problem is Vincent's quiet filiopietism, which precludes realistic assessment and description in an effort to rebut unfair stereotypes of Reconstruction-era black leaders.

Finally, the editors have permitted minor but irritating grammatical errors to pass uncorrected, suggesting to this reviewer that LSU press has not performed up to its normally high standards.

MICHAEL LES BENEDICT
Ohio State University

DANIEL WALKER HOWE, editor. *Victorian America*. Philadelphia: University of Pennsylvania Press. 1976. Pp. 184. Cloth \$15.00, paper \$5.95.

Victorianism, as a concept, helps to shape the image of nineteenth-century English history in a way that is not true of U.S. history. From G. M. Young's classic *Portrait of an Age* to the journal *Victorian Studies*, scholars have used cultural patterns roughly identified with the era of Victoria's reign to define an historical period. Since periodization has enormous consequence for the dramatic structure, and thus meaning, of history, we might ask ourselves whether the traditional Civil War break causes us to lose sight of important dimensions of our past. Daniel Walker Howe, the editor of this book, raises this issue when he argues that by taking the whole period as a unit we gain insight into what he terms the crucial transformation in American life.

Although the book is titled *Victorian America*, Howe's introductory essay makes the important point of viewing Victorianism as a *culture* within

American *society*. While insisting that Victorian culture "exercised a kind of hegemony during the period," he admits the existence of rival and complementary subcultures. Unfortunately, a great weakness of these essays, with the notable exception of Stanley Coben's concluding one, is that nothing is said of contact, conflict, and accommodation among American subcultures.

The volume begins with overviews by Howe and by Richard D. Brown. Both essays take modernization as their theme, and they are intelligent if unsurprising. Howe's essay, which is the richer of the two, shows how much of the recent literature can be subsumed under the rubric modernization. Both essays, however, reveal the Whiggish history modernization theory seems to produce.

The next seven essays narrow the focus to specific topics. Gregory H. Singleton argues that "Protestantism as a system of social organization facilitated the expansion of the American consciousness" from localism to nationalism, "ultimately facilitating the emergence of a corporate society." While there are important connections that can be drawn here, Singleton's model is too simple and his evidence not entirely convincing. D. H. Meyer's exploration of the spiritual crisis of Victorian intellectuals is more successful. Probing the depth of the religious crisis produced by science, he shows how it penetrated the psychological, moral, and esthetic dimensions of people's consciousness. The resulting search for unity, comfort, and inspiration, he concludes, produced a "combination of sentimentalism and intellectualism" in which belief was "tenuously established, floridly expressed, elaborately vague." David D. Hall and Geoffrey Blodgett attempt to rehabilitate Victorian intellectuals and politicians. Hall credits the Victorians with the creation of a modern, secular intellectual life in America. While sensitive to the importance of what he calls the "anglo-American connection" in this development, he seems not to notice the movement of the center of America's national culture from Boston to New York during this process. In the most explicitly historiographical of the essays, Blodgett insists that the Gilded Age and its politicians be taken seriously on their own terms. One may not agree with every substantive point he makes, but he convincingly demonstrates the dangers inherent in treating the Gilded Age as merely an unfortunate prelude to twentieth-century liberalism. There are interesting essays by Claudia D. Johnson on prostitution and the theater, by Dee Garrison on the immoral fiction condemned by librarians, and an exceptionally careful and informative study of Boston's hospitals by Morris J. Vogel. While each is developed within rather tight limits, these essays all suggest some of the complexity and fracture

lines within Victorian culture. The collection concludes with Stanley Coben's concise study of the assault on Victorianism by academics, intellectuals, feminists, blacks, and other ethnics in the early twentieth century.

These essays were originally published in a special edition of the *American Quarterly*. There are thus gaps in coverage and an eclectic quality perfectly acceptable in a journal but rather disappointing in a book. While nearly every essay is effective enough on its own terms, those terms are, with the exception of essays by Howe and Coben, considerably narrower than the issue of whether Victorianism can be a useful historical concept in American studies. The book, alas, is less than the sum of its parts.

THOMAS BENDER
New York University

BRIAN W. DIPPIE. *Custer's Last Stand: The Anatomy of an American Myth*. (University of Montana Publications in History.) Missoula: University of Montana Press. 1976. Pp. xii, 214. \$7.95.

The defeat of the Seventh U.S. Cavalry which we popularly call "Custer's Last Stand" has attracted more history buffs than Sioux to the Little Big Horn, and usually they have been twice as deadly. What more should be said—needs to be said—about this last decisive victory of the Plains Indians over federal forces? Brian Dippie has now made an impressive exploration of the folk and popular legends arising from this famous defeat; and he has done so with the rigor and sense of direction of an academically trained professional historian. His subject is not what happened at the Little Big Horn, but what people thought happened, and consequently what poems, novels, paintings, and movies they constructed about it.

The book has six chapters: the first summarizes the public's initial shock at receiving the news of the disaster, and then white America's rationalizations and recriminations; chapter 2 surveys the poems which the debacle inspired, necessarily paying scant attention to each; the third chapter discusses the more important paintings and illustrations, frequently placing them in their tradition; in chapter 4 Dippie establishes a taxonomy of narrative types and draws upon some nonfiction and printed legends as well as "novels"; re-enactments of the battle, live and on film, are the subject of chapter 5; and the final chapter illuminates the mythic quality of the man and his final engagement.

The discussion of the Custer movies and paintings is especially valuable; after all, they have been most influential in establishing the myth. Three appendixes annotate the major Custer scholar-

ship, and Dippie wisely limits himself to the more important contributions to Custeriana.

The result is a volume of impressive scholarship in an area where it is badly wanting. The volume is marred only by an indifferent publisher's inattention to the finished book, and a copyeditor's demolition of footnote consistency in the epilogue. But the finished book is an important contribution to an understanding of the response to this event, so often ignored by historians and so long cherished by the American people. The book's value lies mainly in the sources listed within the text and the appendixes, and its compilation of poetry and fiction about the event.

Is Custer worth it all? The question is meaningless to the Custerphile; the historian will have to answer on other grounds, for what we are concerned with is a trivial national tradition, an insignificant event the American people have never forgotten. As we reinterpret the bases by which we judge historical importance, we may yet come to see that "popularity" is not synonymous with "triviality."

BRUCE ROSENBERG
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Chicago*

HAROLD K. STEEN. *The U.S. Forest Service: A History*. Seattle: University of Washington Press. 1976. Pp. xvi, 356. \$15.00.

In this study, Harold K. Steen investigates the years from 1876 to 1960 during which the Forest Service grew to national prominence and developed the policies that prevailed during its first hundred years. Rather than write a comprehensive history or cover a broad range of topics already presented in such books as Samuel T. Dana's *Forest and Range Policy* (1956), Steen concentrates on selected internal problems within the top administration of the bureau and in its relations with the lumber industry and others most affected by its policies. Although the Forest Service provided generous financial support for Steen's research over a four-year period and cooperated fully in making official records and personnel available to him, it is obvious that the author retained his scholarly independence.

Steen discusses all administrations but emphasizes those of Franklin Hough, Bernhard Fernow, Gifford Pinchot, Henry Graves, Ferdinand Silcox, and Richard McArdle. State and private forestry, research, and grassroot activities receive little attention. In addition, the author avoids topics that have been adequately treated elsewhere; for example, he devotes only two pages to the Ballinger-Pinchot controversy. Discussion of management of rangeland, wilderness preservation, outdoor recre-

ation, and other concerns recur throughout the book. Because the organization is based on successive administrations of chiefs of the bureau, it is easier to understand the complexity of issues at a given point in time than it is to trace the evolution of policy on a given issue through several decades.

Although Steen does not develop a central theme, he does discuss extensively a question that concerned the Forest Service from the outset: should the bureau cooperate with nonfederal owners of forest lands or regulate the use of their lands? He stresses the importance of the Clarke-McNary Act (1924) in establishing the principle of cooperation, and he explains how the lumber interests resisted government efforts to regulate them.

In his concluding chapter, Steen notes that the Forest Service entered a new era, following passage of the Multiple Use-Sustained Yield Act of 1960, in which it encountered sharp criticism from environmentalists for its policies on clearcutting, timber supply, and other issues. Because of increasing ecological concerns, the bureau is being forced to re-evaluate its goals and methods. Steen's study should contribute significantly to such a re-evaluation.

DOUGLAS STRONG
San Diego State University

W. G. KERR. *Scottish Capital on the American Credit Frontier*. Foreword by CHARLES WILSON. Austin: Texas State Historical Association. 1976. Pp. xiv, 246. \$13.00.

In his foreword, Charles Wilson aptly describes this book as a useful, "realistic" study, whose author, "roused by journalistic experience," identifies the sources of some of the eight million pounds "which the Scots put into Texas mortgages in the 1880's." He also sheds light on the specific nature of the process by which overseas investment took place in the late nineteenth-century international economy. W. G. Kerr makes this contribution by distinguishing between the fairly solid record of the mortgage firms he is studying as business enterprises and the more numerous, better-known cattle-raising firms that underwent the boom and bust experience on the cattlemen's frontiers of that period. He offers three detailed case studies of such land and mortgage investment firms, focusing on Texas as a representative "microcosm of the areas sought out by British companies in America." He piles detail upon detail about the personalities involved and about the relationships between promoters, investors, and managers.

This absorption with the trees may have impaired Kerr's ability to make sense of the forest; the level of analysis and judgment is often unim-

pressive. Kerr's prose is vigorous, sometimes rather purple, but his discussion of the firms as economic enterprises, of incentives for investment, and for using "leverage" in holding control of shares that were subject to calls for further capital is bereft of any economic theory. He claims, for example, to offer a corrective to part of the work by Turrentine Jackson, Clark Spence, and others on the profitability of British investments in the American West, but provides no real evidence on profits and losses in the aggregate, or appreciation of compound interest on alternative forms of investment. When he moves away from his companies to the broader context or to interpretation of data, he makes errors or tosses off dubious generalizations. Thus the introductory chapters should be skimmed lightly and the rest of the book read with both care and caution, separating the valuable, often unique, information about specific enterprises in Texas from the digressions and conclusions that seem either uncertain or irrelevant.

MORTON ROTHSTEIN
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Madison

ALBRO MARTIN. *James J. Hill and the Opening of the Northwest*. New York: Oxford University Press. 1976. Pp. xii, 676. \$19.50.

In the heroic age of capitalism James J. Hill must be counted as one of the "good guys," along with men like John Murray Forbes, Andrew Carnegie, and Cyrus McCormick—as opposed to the "robber barons" such as Cornelius Vanderbilt, John D. Rockefeller, Daniel Drew, and Jay Gould. Despite Hill's importance, there is little written about him. He lacked the color of the Goulds and Rockefellers and the philosophy of Carnegie. Hill stuck to business, avoided politics, and his public pronouncements were largely made at county fairs on the subject of improved agriculture. Hill was highly moral in both his business and private life. He was "opposed to stock jobs on principle" (p. 98), disliked overcapitalization and stock watering (p. 228), and he was shocked at the antics of Victoria C. Woodhull, "the female champion of Free Love & Free Lust" (p. 111).

Until Albro Martin's book there has been only one full-scale Hill biography; that was published shortly after Hill's death in 1916 by the former editor of Hill's St. Paul newspaper, Joseph Pyle. Martin's splendid biography makes up for all the previous lack of attention. He bases his work entirely on primary sources, drawing heavily on the large body of Hill's papers deposited in the James J. Hill Reference Library in St. Paul, Minnesota.

Martin traces Hill from his rural Canadian beginnings to his emergence as one of America's

foremost railroad presidents. In 1856 Hill left his native Rockwood, Ontario in search of opportunity and landed in St. Paul where he remained until his death five decades later. Starting as a shipping clerk on the town's Mississippi docks, he later became a freight forwarder and then an owner of steamboats on the Red River of the North. He made big money stimulating the mass consumption of coal in Minnesota, and he used that capital to buy into railroads. In 1877–78 Hill combined with several associates, including the Mississippi and Red River steamboat baron, Norman Kittson, and two Canadians, Donald Smith (the First Baron Strathcona and Mount Royal for his leadership in the building of the Canadian Pacific) and George Stephen (later First Baron Mount Stephen) of the Bank of Montreal to buy the bankrupt St. Paul and Pacific Railroad, which had just begun to expand from the Twin Cities toward the Dakotas.

Hill changed the name of his railroad to the St. Paul and Manitoba and extended it northward to Winnipeg just in time to cash in on a surge of immigration into western Canada. Hill then bested competing railroads such as the better situated Chicago, Milwaukee and St. Paul; the Chicago and North Western, and the ill-managed Northern Pacific. In 1889, Hill reorganized his railroad as the Great Northern as part of a successful drive to extend it to the Puget Sound. Hill later joined with J. P. Morgan to gain control of the Northern Pacific and the Chicago, Burlington, and Quincy Railroads, but was prevented from formally merging these by the Supreme Court's famous Northern Securities decision in 1904.

Martin attributes Hill's success to a better understanding of the nature of railroad transportation and of growth trends than that of his rivals. Hill built better lines with lower gradients and heavier steel rails; thus he could move traffic in great volume at the lowest costs. As a manager, Hill did not pioneer. He found it impossible to delegate authority and responsibility, and he oversaw much of the construction and railway operations himself.

Martin provides extensive coverage of the little-known aspects of Hill's life, especially his role in the development of Minnesota and Manitoba. He devotes relatively less space to celebrated episodes such as Hill's fight with the Union Pacific's Edward H. Harriman, the Northern Pacific stock-market "corner" of 1901, and the Northern Securities case. Martin is sympathetic with Hill, but properly skeptical. Hill occasionally lied—for example, when he told the citizens of Spokane that the Great Northern would end rate discrimination against their city. Martin's book is exceptionally well written and will certainly rank as one of the

best biographies of a nineteenth-century business leader ever written.

STEPHEN SALSURY
University of Delaware

GENEVIEVE C. WEEKS. *Oscar Carleton McCulloch, 1843-1891: Preacher and Practitioner of Applied Christianity*. Indianapolis: Indiana Historical Society. 1976. Pp. xvii, 248. \$7.50.

Although neglected by historians of the Social Gospel, Oscar C. McCulloch now appears as an important figure in the early stages of that movement. Genevieve C. Weeks effectively interprets him as a man of extremely wide interests but with two dominant passions: making the Plymouth Congregational Church of Indianapolis, which he served from 1877 to 1891, an "open," or "institutional," church that served the various social classes of its community, and promoting the expansion, diversification, and more efficient (i.e., "scientific") organization of charities. Had his career not been cut short when he was only forty-eight, he would almost certainly have received national recognition when the Social Gospel reached its apogee.

Weeks judiciously assesses McCulloch's position in the spectrum of contemporary religious opinion, showing him to have shared the essential moderation of his fellow Congregationalists, Washington Gladden and Lyman Abbott. She also sensibly evaluates his programmatic originality, contending that, though he was not the first in either instance, he was one of the earliest to adopt "institutional" church programs and to create a Charity Organization Society. A specialist in social-service history, Weeks is especially acute in treating McCulloch's charitable work in Indianapolis and beyond. Despite the seemingly harsh differentiation between the "worthy" and "unworthy" poor that informed its procedures for investigation, centralized record-keeping, and elimination of duplication in assistance, she interprets the organized charity movement sympathetically as a foundation for modern social work.

Several aspects of McCulloch's career receive less satisfying examination, but this is due to deficient sources rather than to the author's neglect. Because his published writings were practical rather than theological, and because he did not write out his sermons, the contours of McCulloch's theological liberalism are fuzzy. Limitations of the sources also probably account for limited treatment of his congregation's relationship to the social services that McCulloch launched at Plymouth Church. The question remains, in his case as in others, whether the Social Gospel was ever compelling to many parishioners.

This work is a welcome addition to the literature on the Social Gospel, adding to the still small but growing number of case studies that go beyond the general surveys of Hopkins, May, and others.

JACOB H. DORN
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ALAN DAWLEY. *Class and Community: The Industrial Revolution in Lynn*. (Harvard Studies in Urban History.) Cambridge: Harvard University Press. 1976. Pp. viii, 301. \$17.50.

Class and Community: The Industrial Revolution in Lynn by Alan Dawley is an inquiry into the development of working-class consciousness in the nineteenth-century shoemaking town of Lynn, Massachusetts. Shoes were made by artisans in household shops until the early nineteenth century when fortune-seeking capitalists seized upon the opportunities presented by a growing national economy to restructure the industry by introducing the putting-out system and the central shop. The artisans' resentful reaction to their loss of status and influence was expressed in a local newspaper, the *Awl*, whose editors called for "Equal Rights," by which they meant the fair distribution of wealth, power, and status among those who contributed to the community's economy. In spite of the doctrine of equal rights, however, the power of the capitalists accumulated, culminating in the late 1850s with the introduction of the sewing machine, which in turn made possible the appearance of the factory system during the 1860s.

Dawley's chief concern is to explain the reaction of workers to these changes in Lynn and thereby to shed light upon the development of the American labor movement as a whole. He focuses particularly on two related issues: the historic importance of the workers' equal rights concept and the reasons why European-style socialism failed to take hold in the United States. In the course of his argument he takes to task the John Commons school of labor historians for misinterpreting the course of labor organization in the boot and shoe industry because Commons was ideologically committed to the notion that workers and owners could negotiate as equals within the capitalist system. Dawley is not free of ideological commitments himself, however. He ascribes great social virtues to the preindustrial household economy and is clearly unhappy that America has ignored socialism. Dawley expresses these opinions colorfully, if somewhat heavy-handedly, through the use of metaphorical language and strong imagery. This is a book in which capitalists literally drool while contemplating profits.

Labor protest in Lynn culminated in the Great Strike of 1860 and coincided with the introduction

of the sewing machine, which displaced many female binders. But, Dawley argues, during the crucial decade that followed, while the factory system was taking hold, workers got sidetracked by the Civil War. The war caused them to direct their protest against the southern slave-holders rather than their "class enemy," the local industrialists. Furthermore, workers were coopted into a political system that appeared to be democratic while it really favored the capitalists' interests. "The ballot box was the coffin of class consciousness" (p. 70). Lynn developed a strong Knights of St. Crispin organization in the 1870s and was a bastion of the Knights of Labor in the 1880s. In addition, a local Workingmen's Party won control of the city hall in the late 1870s and 80s. The party was not really radical, however, and failed to create "a new working class style of its own" (p. 201). Why the workers achieved so little, in spite of the strong base for working-class solidarity in Lynn, is the main theme of this book. Its value will rest upon the validity of Dawley's interpretation of labor history rather than upon his exposition of the history of Lynn, because the book does not delve broadly into the overall development of the town.

CAROL HOFFECKER
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and was limited. The chapter on the modernization of Buffalo's schools and the pioneer efforts of Buffalo's Charity Organization Society exemplify these limitations.

Shelton's final chapter sums up the impact of the decade on Buffalo's elite reformers. Her conclusion is that reform weakened the elite's control of the city. Though progressive change took place in several areas, reformers had ambivalent feelings about the consequences. The problems of ethnicity, poverty, corruption, and growth remained. Meanwhile, new groups challenged elite dominance, and the transition to pluralistic politics in Buffalo began. Thus, the decade marked the beginning of a major shift in political control in Buffalo, in effect modifying both Hays' and Wiebe's theses regarding business and new middle-class dominance in the Progressive Era. One wishes Shelton had carried her thesis into the twentieth century for a larger perspective, but that was beyond the boundaries of the dissertation. Still, could she have gone on to the outbreak of World War I before publication, her valid point might have developed into a major reinterpretation of the era.

JAMES B. CROOKS
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BRENDA K. SHELTON. *Reformers in Search of Yesterday: Buffalo in the 1890s*. Albany: State University of New York Press. 1976. Pp. vii, 245. \$12.50.

If urban history may have stages analogous to those of Sam Warner's Philadelphia, Brenda Shelton's study of Buffalo fits between the obsolete urban filiopietism of nineteenth-century city historians and the avant garde of what some scholars call the new urban history. She examines the process of reform in Buffalo primarily from the perspective of the community's leaders.

Shelton begins by briefly describing the character of the city in the 1890s. She examines Buffalo's elite from whom the reformers came. Here she incorporates the views of Samuel Hays and Robert Wiebe, recognizes the dominance of WASP culture, and introduces the rising influence of Jewish and Catholic leaders among the Social Gospel Protestants. She contends that Melvin Holli's division between structural and social reforms in Detroit does not describe Buffalo. She finds that the Hofstadter status thesis does not apply. She concludes with Commager that the decade was a watershed for Buffalo as well as for Americans.

Reform began in Buffalo with attacks on the bosses. The issues of home rule, a new charter, factory regulation, public health, child labor, and tenement conditions are familiar. As elsewhere, reform benefited middle-class Americans most,

LAWRENCE GOODWYN. *Democratic Promise: The Populist Moment in America*. New York: Oxford University Press. 1976. Pp. xxvii, 718. \$19.95.

In the scholarly literature Populism often appears as a dependent variable to be explained by impersonal socioeconomic forces. Lawrence Goodwyn's account stands outside that tradition. He describes Populism from the inside, "letting the movement culture of Populism narrate its own flowering." From that narrative emerges a protest movement with a closely reasoned critique of America's political economy and a consistent plan for altering power relationships in an industrializing nation. "The issues of Populism were large," Goodwyn asserts. "They dominate our world."

Goodwyn's analysis proceeds from a monetarist interpretation of post-Civil War economics, in which the key event is the prolonged contraction of the money supply. From a monetarist perspective, the Greenback movement which emerged during that great contraction offered a radical critique of emergent finance capitalism and a means of alleviating the suffering of "producer-debtors." In Goodwyn's view Greenbackism informed the Populist experience.

In the tradition of C. Vann Woodward, Goodwyn finds the origins of Populism in the Texas-based National Farmers' Alliance and Industrial Union, commonly referred to as the

Southern Alliance. The Alliance's ambitious program of cooperative purchasing and marketing attracted masses of farmers because it attempted to revolutionize agricultural credit arrangements. Subsequently, the cooperatives' failure, explained within the "movement culture" of the Alliance by its Greenbacker leaders, further radicalized the movement and generated support for the subtreasury plan and political insurgency. The People's Party and its Omaha Platform of 1892 emerged directly from the radicalized Alliance. Indeed, Goodwyn argues, "Alliance radicalism was Populism" (p. xx).

Goodwyn contrasts this agrarian crusade with what he calls the "shadow movement" of Populism, which began with the "Northwestern" Farmers' Alliance and matured in the free silver campaign. Goodwyn shows that the Northwestern Alliance was ephemeral and was supplanted by the "Southern" Alliance in most states. He blasts the silver campaign as spurious, financed by silver miners, and manned by political "trimmers." The climactic struggle between silverites and mid-road Populists in 1896 was, therefore, a contest between a sham movement and authentic Populism.

This is an important book. Goodwyn has, more carefully than any other student of Populism, traced the Greenbacker thread that runs through the agrarian crusade. The core of the book is Goodwyn's extremely perceptive analysis of the agrarian movement in Texas, particularly the development of the Alliance between 1885 and 1888. The focus on Texas, and to a lesser extent on Kansas, enables him to identify and interpret the role of the movement's left wing more clearly than has been done before. From this analysis of Texas Populism Goodwyn's interpretation emerges: the Alliance's cooperative experience, in the presence of Greenbackism, yields the politics of the subtreasury, which yields the People's Party and mid-road Populism. This hypothesis is supported by a great deal of evidence, both in this study and elsewhere in the literature. Unlike the ahistorical brand of structural-functionalism which has been applied to Populism since the 1950s, it forces one to look at Populism's development over time without assuming *a priori* that its adherents were irrational.

Democratic Promise replaces John D. Hicks' 1931 study, *The Populist Revolt*, as the standard history of Populism. Hicks focused on the "shadow movement" and did not fully understand the dynamics of the more radical movement that is the object of Goodwyn's work. Nevertheless, this new history of Populism leaves unanswered some important questions. Before accepting the claims of the Populists (and Goodwyn) about the causes of rural distress, we must know more about such things as price structures, terms of trade between farm and

nonfarm sectors, and U.S. involvement in the world agricultural market. Furthermore, Goodwyn's suggestion that Populist monetary programs would have benefited most Americans (as distinguished from most farm operators) is not self-evident.

Other questions arise from Goodwyn's assumption that Texas Populism (or perhaps Texas-Kansas Populism) was normative for the entire movement and, as a result of this assumption, from his lack of sustained grass-roots analysis beyond Texas and Kansas. To be sure, Populist missionaries from Texas fanned out over the South and West carrying the new gospel, but what they said and what their audiences heard were often different. Goodwyn reveals little of the great variety of the Alliance's cooperative experiences or of the non-Greenbacker sources of its politics. His accounts of local Alliance activities, although generally accurate, contain some misinformation, notably about Georgia, Kansas, and North Carolina. Furthermore, we learn little about how Populists at the grass roots related to the web of social and economic relationships in rural America. As Goodwyn says, Alliance-Populist leaders spoke to "the whole people," but what sort of people heeded the call? There is evidence that in the South the Farmers' Alliance mobilized a cross section of rural whites, but that the third party upheaval of 1892 divided communities along lines of class, as well as race. Particularly after the emergence of the People's Party, class analysis of the movement is essential, Goodwyn's assertion to the contrary notwithstanding.

That this study leaves unanswered, and even unasked, major questions about American Populism does not negate its remarkable contributions nor its indispensability as a guide to further research.

This is an impassioned book. It argues that Populism could have transformed American life for the better. So be it. Populists were impassioned men and women who believed they could make America a more humane and democratic place. This is also a deeply pessimistic book that begins by announcing its subject to be "the decline of freedom in America." And yet the author is the same Larry Goodwyn who once edited, with some optimism, that humane and authentically Populist journal, the *Texas Observer*. His book, as its dust jacket notes, "is grounded in the faith that the democratic ethos is not yet fully compromised."

ROBERT C. MCMATH

Georgia Institute of Technology

KAREL D. BICHA. *Western Populism: Studies in an Ambivalent Conservatism*. Lawrence, Kans.: Coronado Press. 1976. Pp. 163. \$8.50.

This is an intriguing collection of essays, several of which have already been published. They are provocative but scarcely persuasive. It is Karel Bicha's thesis that Western Populists were essentially conservative, primarily committed to a free-market economy, limited government, and state sovereignty. He attempts to substantiate this contention first by analyzing four prominent Populists: Jerry Simpson, William V. Allen, L. D. Lewelling, and Davis Waite. Bicha interprets their political actions with obstinate hyperopia, but even he does not suggest that any of these was a representative Populist, calling Allen, in fact, "a non-Populist."

In an attempt to make legitimate generalized statements about Populists, therefore, Bicha next examines the legislative activity of Populist state legislators in seven Western states. He concludes that Populists were no more likely to introduce or support reform measures than were non-Populists and that, in another indication of their basic conservatism, the scope of their reform interests was more limited than that of non-Populists. But questionable methods, unreasonable definitions, source limitations, and strange decisions in using those sources render these conclusions dubious at best. First, in identifying "reform" measures, Bicha considers merely the titles of bills, not their substance, thereby ignoring political practice and legislative reality. This he compounds by partially excluding similar bills, thereby minimizing Populist reform activity, as in the 1891 Nebraska House where Populists introduced eight (of nine) bills to regulate interest rates but receive credit for only one. Finally, and with even less justification, Bicha exercises an extraordinary selectivity in the bills he does consider, as when he declares, for the same legislature, that only non-Populists introduced bills for labor and electoral reforms, overlooking seventeen such Populist bills. In other matters, too, he ignores Populist proposals altogether, incorrectly attributes them to Republican authorship, or carelessly misrepresents legislative maneuvers and outcomes.

These massive, and ultimately decisive, shortcomings are unfortunate, for Bicha has isolated a real problem in the frequent disparity between Populism and some Populist politicians, but that problem has a far more satisfactory explanation within the political system itself.

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JOHN HIGHAM. *Send These to Me: Jews and Other Immigrants in Urban America*. New York: Atheneum. 1975. Pp. ix, 259. Cloth \$10.00, paper \$4.95.

One of John Higham's abiding concerns has been the dilemma of national unity and ethnic diversity in American society. It is this concern which gives thematic coherence to these writings drawn from over two decades of scholarship. Although, with one exception, previously published, these collected essays, revised and expanded, are a welcome and valuable addition to the literature of ethnic history. The volume bears the Higham hallmark of literary and historical craftsmanship.

Higham is best known as an astute analyst of the national psyche, a cartographer of the ideological geography of America. As in his major work, *Strangers in the Land*, Higham in these essays probes the feelings and thoughts Americans have had about ethnic and religious differences. Specific studies deal with the politics of immigration restriction, the Statue of Liberty as symbol, the nativist Henry F. Bowers, and the novelist Abraham Cahan. Three articles focus on the sources and character of anti-Semitism in America. Higham's special contribution to this historical controversy is his cogent argument that social discrimination experienced by Jews was less a product of ideological anti-Semitism than of the status rivalries of the urban middle class. Higham expanded on this insight in a groundbreaking article, "Another Look at Nativism," first published in 1958. Rather than more studies of nativist ideology, he prescribed the historical analysis of the social structure, particularly of the relationships between classes and ethnic groups. Ethnic competition for status and power, he observed, had been an everyday fact of life in America. Group antagonisms were more often the result of such "basic structural realities" than of irrational prejudice. By recognizing cultural differences and status conflicts as realities, Higham as much as anyone defined the research strategy of the new ethnic history.

In three recent essays, "The Immigrant in American History," "Ethnic Pluralism in Modern American Thought," and "Another American Dilemma," Higham joins the current debate over the significance of ethnic diversity for American society. Though set forth in measured, reasoned terms, these pieces by their nature are argumentative and controversial. The author candidly states his point of view as "a general historian of English descent, possessing only a distant sense of affiliation with the immigrant experience." Admittedly he looks askance at ethnic differences and feelings as potentially dangerous and destructive. Identifying with the host society, Higham affirms that not all Americans are ethnics. ("Ethnic history," he adds, "should not overlook the nonethnics.") Still he accepts and approves of ethnic groupings as a permanent feature of American life. Finding both integration and pluralism unrealistic and

morally objectionable, Higham embraces "pluralistic integration" as "our best tradition" in ethnic relations. Briefly, this model envisions a unifying common culture which, however, respects and sustains "the efforts of minorities to preserve and enhance their own integrity." Ethnic nuclei are strengthened, but boundaries are permeable; thus both integration and ethnic cohesion are served.

Pluralistic integration appeals as a model of ethnic relations because it recognizes the value of both unity and diversity. As explicated by Higham, however, the theory rests upon certain assumptions to which this reviewer must take exception. Higham posits (but does not clearly define) several key concepts, among them "host society," "common culture," and "core population," which presumably refer to nonethnic America. But from whence do these derive? In his discussion of immigration history, Higham classifies the English colonists as "founders" and not immigrants, since they were the formative element of the new society. (To this reader, Higham appears to strain in his effort to demonstrate the English preponderance in colonial America, the limited impact of subsequent immigrations, and the erosion of ethnic consciousness, but a discussion of these topics is beyond the scope of this review.) Although this summary can hardly do justice to the subtleties of the argument, Higham appears to conceive of an American majority composed of those of English ancestry plus all others who have been assimilated into the "host society." In a telling image, he suggests that "American society may be visualized as a cluster of immigrant-ethnic communities lapped by an expanding core population of mixed origins and indeterminate size." Within such a conception, ethnicity is clearly associated with minority status, foreign culture, and immigrant origins. To this reader, such a dichotomous view of American society appears to be profoundly ethnocentric.

By juxtaposing majority and minorities, nonethnics and ethnics, core and marginal, Higham seems to be indulging in that "absolutization of social identities" which he elsewhere criticizes. Such categories also suggest qualitative differences and hence invidious comparisons in Americanness. Of course, one can argue that an Anglo-American society in contradistinction to immigrant groups did exist as a discrete entity in the past, but to speak in terms of such a duality in the present tense seems anachronistic. In any case, Higham has gone beyond historical analysis and has endowed his model with normative value. In the light of the foregoing critique, pluralistic integration appears to be a strategy for perpetuating the ethnic status quo. Conceptualized as a solar system, the core culture is the sun, and the minor-

ity cultures are the planets, each different and seemingly autonomous, but in reality controlled by the central body. Ethnic diversity is to be tolerated and even nourished, yet it also is to be "modulated" by the majority culture. The power relationships inherent in "pluralistic integration" are implied, rather than explicit. Higham's discussion of ethnic relations lacks a systematic analysis of the basic structural realities of pluralistic America. Ethnicity itself is dealt with primarily in ideological and psychological terms; its embodiment in behavioral and institutional realities is beyond the scope of his analysis. For this reason perhaps, Higham's theory of pluralistic integration is subject to the same criticism that he leveled at the old theory of cultural pluralism: "It stressed values to the point of obscuring realities of ethnic status and power."

What comes through clearly in these essays is Higham's primary commitment to national unity. "Hanging together" is his overriding concern. In a recent article bearing that title, Higham described the desiccation of the American ideology in recent times and its obsolescence as an integrative mechanism for the society. The revitalization of a common faith, a unifying ideology, a shared culture, is imperative, he believes, for a decent multiethnic society. Only a separatist, presumably, would disagree with this proposition. However, an ideology which begins by making invidious distinctions between ethnics and "other Americans," and which contemplates the continuing dominance of the "majority" over ethnic "minorities" is bound to be more divisive than unifying.

In this volume, Higham has defined with precision the need to reconcile ethnic diversity with national unity in American life. With erudition and skill he has illuminated many aspects of the problem, past and present, and he has presented an eloquent argument for pluralistic integration as the best means of resolving the dilemma. Those who differ have a responsibility to offer alternatives.

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CECYLE S. NEIDLE. *America's Immigrant Women*. New York: Hippocrene Books. 1976. Pp. 312. \$4.95.

Cecyle S. Neidle has chosen to write a book about the history of immigrant women after the fashion of the laudatory "great contributions" we used to read as children. She seeks to convince us that "foreign-born women, no less than men, played distinctive roles in American society." The narrowness of this perspective limits the scope and

usefulness of the book and threatens in the end to trivialize the women who populate its pages.

America's Immigrant Women has twelve chapters, each centered on a different theme—religion, trade unions, music—and each of which attempts to develop the contributions of women who immigrated to America. Each chapter runs the gamut of time. For example, the chapter entitled “Social Progress” begins with Mary Wollstonecraft, who wrote the *Vindication* in 1792, and concludes with Rosika Schwimmer, who died in 1948. The result is a series of biographical vignettes. Some of these, like that of the Croatian saloon keeper, Manda Evanich, are fascinating. More often, they merely outline lives spent in useful service. The evidence, as the author argues, adds up to a powerful case for the contributions of foreign-born women to “national progress.”

The book might be useful for students and young people who still need to be convinced that there were great women, but most historians would have difficulty taking it seriously. Its questions are framed in a language long rejected by scholars of women's history. It judges women, for example, by the standard of achievement in a male-defined world. It isolates and glorifies individual women, rather than focusing on group process or collective action. The author repeatedly ignores her own definition of immigrants as those who faced the necessity of “adjusting to a new mode of existence and accepting alterations in life-style.” Women like Wollstonecraft (who never came to America), and Dorothea Dix and Margaret Fuller (who were born here), receive attention. Missionaries like Evangeline Booth, visitors like Madame Blavatsky, and women who were babes-in-arms on arrival are treated as though they experienced the same kinds of transitions. There is no sense of how the immigrant experience might have affected a woman's world view—no attempt to assess the quality or style of adaptation for those who came in steerage as compared to those who came as daughters of the rich, travelling like the Blackwell sisters with servants and a governess.

Even in its own terms, *America's Immigrant Women* is unsatisfying. It begs the question of what makes the contribution of an immigrant woman unique. It contains peculiar and inexplicable emphases. The author tells us, for example, that she had no space for teachers, lawyers, judges, or creators of the visual arts. Yet she devotes two separate chapters to musicians and music teachers. Why were these more important than great dancers like Suzanne Dauvillier and Marie Bonfanti, great actresses like Agnes Booth, Marie Dressler, or Alla Nazimova, or artists like the sculptor Elisabeth Ney? Minor but annoying discrepancies appear throughout. The caption under the photograph of

Anzia Yezierska who died in 1970 tells us that she lived until 1973. Rose Pastor Stokes was born in 1879 on page 174, and in 1889 according to her photograph's caption. The bibliography is wholly inadequate: of fourteen selected memoirs, only four of those listed were written by women.

All of this is unfortunate for we badly need insight into the experience of the millions of women who came to America as immigrants. The author has pulled together a substantial quantity of information. It is sad that she never moves beyond simple compilation.

ALICE KESSLER HARRIS
Hofstra University

MARK REISLER. *By the Sweat of Their Brow: Mexican Immigrant Labor in the United States, 1900–1940*. Westport, Conn.: Greenwood Press. 1976. Pp. xi, 298. \$14.50.

Mexican immigration to the United States has long been a controversial subject—its literature polemical, its statistics misleading. Ever since the pioneering studies of Paul S. Taylor (an economist) and Manuel Gamio (an anthropologist), the topic has eluded historians. Mark Reisler's study deals with the period from the arrival of Mexicans in the United States at the beginning of the twentieth century to the attempts made by federal and local officials to be rid of them in the Depression years. He describes in detail how immigration policy worked for and against Mexicans, their importance in agriculture and industry, the stereotyped views anglos held about them, and the efforts to establish a quota limiting their entry.

Faced with skyrocketing cost of living, an unbalanced wage differential, and continued political upheaval, Mexicans came to the United States in unprecedented numbers. They found employment primarily in the seasonal agricultural harvests of the Southwest and on the railroads as maintenance workers. Mexicans were also employed by beet-sugar companies as far north as Ohio and Minnesota, by the steel industry in Pennsylvania, Illinois, and Indiana, and even for a time as cotton pickers in Mississippi. Their employers claimed that Mexicans were “docile,” willing to work long hours for low wages. On the other hand, labor leaders and nativists were alarmed at their continuing influx and attempted to amend the 1924 Quota Act to include the western hemisphere, with Mexico as the chief target.

This is no impressionistic survey. Reisler's research is solidly based on manuscript sources in the National Archives, Library of Congress, and elsewhere. The views of the various factions come alive through judicious quotations; Taylor's and Gamio's published studies, as well as the volumi-

nous popular writings of the 1920s, provide opinions from the Mexicans themselves, their employers, and a broad range of nativists, politicians, and labor leaders. Reisler did not make use of the George P. Clements papers at UCLA (though he mentions Clements), a source that could have accorded a larger role to the Los Angeles Chamber of Commerce than this study provides. Mention might have been made of the Cristero Revolt as a possible factor in stimulating emigration from Mexico in the late 1920s. There is also little notice given to the size and attitudes of the Mexican-American (native-born or native-born) population. These are, however, minor points.

The time is long past due for the authors of college level history textbooks to include the contributions of Mexicans and Mexican Americans in the mainstream of American history. This solid work of scholarship gives notice that discussion of the restriction controversy of the 1920s must include the role played by Mexican immigration and labor.

ABRAHAM HOFFMAN
Los Angeles Valley College

HAROLD W. CURRIE. *Eugene V. Debs*. (Twayne's United States Authors Series.) Boston: Twayne Publishers. 1976. Pp. 157. \$7.95.

Up to a point, this concise and meaty little volume will serve the general reader well as an introduction to the man who remains, more than Daniel DeLeon or even Norman Thomas, America's most influential and attractive socialist. Given the short space allotted to him Harold Currie has sensibly eschewed any attempt to discover new facts about Debs' life, relying instead on Ginger, Coleman, and other earlier biographers. The author makes intelligent use of Debs' published articles and speeches—as well as some unpublished ones—to examine his views on labor, government, religion, war and peace, prison reform, and other subjects, each in a separate vignette. The book opens and closes with short chapters giving more general consideration to Debs' career and views.

The advantage of this approach is that it gives the reader easier access to Debs' opinions, as well as somewhat lengthier treatment of them, than might be expected in a biography of one hundred and fifty pages. The disadvantage is that it renders the book episodic, ahistorical, and lacking any central or unifying theme. By means of judicious illustration and quotation Currie conveys well what rendered Debs so attractive to his contemporaries—his honesty, his courage, and the down-home flavor of his evangelical socialism. But the episodic, and somewhat over-eulogistic nature of the treatment prevents us from capturing the

darker side of Debs' nature, such as his drinking, or the effect on his wife Kate of his long absences from home. It also exposes the author's failure to place each of the topics which engaged Debs' interest as a reformer into its proper historical context. Debs' early opposition to strikes as editor of the *Locomotive Firemen's Magazine*, for example, is inexplicable without reference to the promulgation of that policy by the Knights of Labor, which was the dominant national labor federation at that time. The same can be said of Debs' views on violence, and other matters. The result is a lack of analytical rigor concerning what Debs' position as a radical actually was. In his conclusion Currie rightly acknowledges that Debs was "an ardent revolutionary"—and in some primitive sense a Marxist—while two pages later boldly announcing that his subject's political philosophy was "very much an American tradition in that it rests on the same base as mainstream Americanism." The author's seeming inability to recognize the contradiction implied in these two statements seriously weakens an otherwise useful book.

JOHN H. M. LASLETT
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Los Angeles*

DAVID S. PATTERSON. *Toward a Warless World: The Travail of the American Peace Movement, 1887-1914*. Bloomington: Indiana University Press. 1976. Pp. xi, 339. \$15.00.

In reform movements, expanded membership and activity does not necessarily guarantee success. This is particularly true when new adherents introduce a variety of motives, preoccupations, and plans that increase the diffuseness of an already unfocused cause. Such a pattern of development, David S. Patterson suggests, accounts for much of the "travail" of the American peace movement between 1887 and 1914.

Diversity, Patterson argues, constituted a salient characteristic of the movement. Before 1900, religious pacifists, secular nonresistants, and free-trade liberals had given the movement "a utopian and anti-establishment dimension." Later, such functional elites as lawyers, educators, and businessmen swayed the movement toward "institutional pacifism." Still, divisions persisted "between imperialists and anti-imperialists, legalists and federalists, and interventionists and non-interventionists." Disunity among internationalists blunted their impact on policy-makers.

Even had peace activists achieved unity, Patterson implies, their elitism would have limited their effectiveness. Socially prominent and often professionally trained, they exalted qualities of self-restraint and educated civility, viewing the man in the street as "an important ally only in proportion

to his ability to absorb their high-minded faith in reason, moral rectitude, and gentlemanly conduct." Such "elitist predilections" precluded effective appeals for broad popular support.

But Patterson does not dwell entirely on internal weaknesses. His most valuable contribution is his exploration of interrelationships between the emergence of new peace organizations, leaders, and concepts, on the one hand, and the currents of foreign policy crises and initiatives, on the other. Drawing upon a massive survey of manuscript sources, he carefully documents the impact on peace activists of major international issues from the Olney-Pauncefote arbitration treaty to the outbreak of World War I. In consistently probing the nexus between event and reaction, Patterson provides our best account of the interplay between peace movement and foreign policy.

Patterson chose the word "travail" to emphasize the movement's "many frustrations and failures." Some of these stemmed from the movement's "ideological diversity." One might argue, however, that such phrases as "travail," "heterogeneity," and "ideological diversity" only partly describe the movement's ambience. Despite personal and organizational rivalries, the leaders were strikingly homogeneous in regional and class origins and in social and political bias (elitist and mugwumpish). Disagreements over disarmament, anti-imperialism, and legalism still fall short of real "ideological diversity." Essentially, all of the leaders glorified American institutions, endorsed the era's pervasive nationalism, agreed on genteel tactics, and distrusted the masses.

Despite frustrations, the peace movement maintained an irrepressible optimism in the face of seeming adversities. Its leaders met disappointments by magnifying "even the most superficial signs of progress" (p. 109). "Travail," with its connotations of painful effort, torment, and suffering, may overemphasize the agonies of a movement which provided "a safe outlet for . . . tepid reform inclinations" of those who "basked in the sunny optimism of the decade" (pp. 132, 140).

The subtitle, "The Euphoria of the American Peace Movement," although also an exaggeration, might serve equally well to describe the increasingly conservative and complacent peace movement before 1914.

ROLAND MARCHAND
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Davis

GEORGE BROWN TINDALL. *The Ethnic Southerners*. Baton Rouge: Louisiana State University Press. 1976. Pp. xvii, 251. \$11.95.

The Ethnic Southerners is a collection of previously published monographs and addresses by the dis-

tinguished historian George Brown Tindall. Tindall needs no introduction to American historians. From his earliest book, *South Carolina Negroes, 1877-1900* (1952) to his compendious survey of *The Emergence of the New South, 1913-1945* (1967), the tenth and final volume in the well-known series, *History of the South*, Tindall's name on a work is enough to announce a judicious, exhaustive assessment of the topic. More than one reader has looked up from his pages and wondered admiringly, is there any source, from the most obscure M.A. thesis to the latest major interpretation, he has not discovered and digested? Invariably measured, though not hesitant, his words are consistently relevant to the major issues scholars have confronted—or should confront. If he has a weakness, it is a function of his great strength: his balanced, calm coverage of a subject tends to flatten out his judgments and rob his thought of sharp contention or penetrating insight. In this he is an unmovable counterweight to the W. J. Cash of Southern history.

Like many such books this one lacks a "central theme," save Tindall's oft-repeated statement that the South, though distinctive (perhaps elusively so), has lived with and furthered the same forces of continuity and change—both the good and the bad—that have shaped the rest of the nation. Several of the essays, notably, "The Benighted South: Origins of a Modern Image" (1964), are running-battles with those, like H. L. Mencken, who have created the image of the South as a reactionary, static Sahara of the Bozart. "Business Progressivism: Southern Politics in the Twenties" (1963) is yet another part of Tindall's rebuttal of myth. Now and then a note of defensiveness creeps in. Mencken is mentioned as often as the New Deal or lynchings, but the note is faintly sounded. Both "Mythology: A New Frontier in Southern History" (1964) and "The Significance of Howard W. Odum: A Preliminary Estimate" (1958), point to indigenous forces of change. Tindall seems to equate change with "progress." His essays rest squarely on the authority of primary research and careful reading. From this perspective, though, the more ambitious essays, including "Beyond the Mainstream: The Ethnic Southerners" (1974), attempt too broad a canvass. But even here he points to new, untapped areas in need of careful scholarship—just the sort Tindall himself has consistently produced.

BRUCE CLAYTON
Allegheny College

RUSSEL L. GERLACH. *Immigrants in the Ozarks: A Study in Ethnic Geography*. (University of Missouri Studies, number 64.) Columbia: University of Missouri Press. 1976. Pp. xii, 206. \$15.00.

Students of white ethnicity in America will find much to attract them in this analysis of immigrant groups in the Ozarks of southern Missouri. Russel Gerlach, a cultural geographer, uses a descriptive approach to determine if the landscape of the region reveals evidences of ethnic culture or if immigrant group distinctiveness has been eroded by decades of assimilation. The author first establishes the spatial and temporal facts of ethnic group migration into the Ozarks and then discloses contemporary evidences on the landscape of their occupancy.

Approximately ten percent of the region's present population may be classified as white ethnic, according to the author's definition. Since eighty percent of these persons are descendants of German immigrants, the bulk of the book consists of information about them. Gerlach demonstrates that Germans in the Ozarks retain distinctiveness in religion, architectural forms, location of farm houses, certain agricultural practices, land use, value of their land, homes, and farm buildings, and in the spatial organization of their villages. In some respects, the contrasts between Germans and Old Stock Americans are striking and reflect an unexpected measure of cultural pluralism in the region.

Most other ethnic groups that settled in the Missouri Ozarks—Swedes, Poles, Italians—were so small in number that little remains on the cultural landscape to distinguish them from other elements of the population. The substantial enclave of French at Old Mines in Washington County is probably an exception. The distinctive character of this group apparently is due to poverty and isolation rather than to conscious efforts at cultural maintenance. In this respect they contrast sharply with the dozen Amish and Mennonite groups that have migrated during the past decade from such states as Pennsylvania, Ohio, and Indiana to establish isolated rural colonies in the Ozarks.

Although Gerlach makes some effort at comparative analysis, only the Germans are studied systematically or comprehensively. He assumes but does not conclusively demonstrate that each ethnic group altered the landscape at the time of its arrival in a way reflective of its culture; similarly, he attributes distinctive group characteristics that remain today to ethnic culture without seriously pursuing other variables. A special strength of the analysis is the author's recognition of the church as the pre-eminent ethnic institution. Numerous illustrations and maps are of high quality, but inclusion of a map that identifies Missouri's counties by name would have been helpful.

FREDERICK C. LUEBKE
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Lincoln

BLAINE A. BROWNELL and DAVID R. GOLDFIELD, editors. *The City in Southern History: The Growth of Urban Civilization in the South*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1977. Pp. 228. Cloth \$13.50, paper \$6.50.

Sustained investigation by professional historians of urbanization in the southern portion of the North American continent is a recent phenomenon. Beginning roughly with the publication of Richard C. Wade's *Slavery in the Cities: The American South* (1964), a group of younger scholars with an abiding interest in Dixie's urban past has haunted the halls of AHA, OAH, and SHA convention hotels, swapping research results, trading news about journal and press interest in the subject, and exchanging information about job openings in "sun-belt" colleges and universities. For almost a decade they remained underground, marginal figures among southern historians more concerned with traditional topics. Yet the young urbanists persisted, pursuing their research and gradually insinuating themselves into back rooms and basements in the mansions of southern history. Now the group is coming out. The devotion in 1976 of an entire issue of the *Journal of Urban History* to urban themes in southern history constituted the first sign of its debut, and the publication of this volume amounts to something like a declaration of the southern urbanists' manifesto.

Dedicated wryly to "our parents, who wanted us to be doctors or lawyers," the book is a contribution to the study of both American civilization and urban history. It consists of six essays, five of them tightly edited pieces which focus on the role of civic leadership in the urbanization process, the relationship of southern cities to the national economy, and race relations. Carville Earle and Ronald Hoffman cover the colonial era and lay out a tidy yet provocative scheme of urban development keyed to transitions in commodity production. David R. Goldfield handles the forty years between 1820 and 1860, moving smoothly from boosterism, railroad promotion, and city growth to urban slavery and municipal government, and concluding with a novel economic explanation of the response of southern urban spokesmen to the secession crisis of 1860-61. Howard N. Rabinowitz treats the last half of the nineteenth century in an essay particularly good on race relations. Blaine A. Brownell examines the first forty years of the twentieth century and, like Goldfield, makes a strong case for the similarity of the urbanization process north and south of the Ohio River. Edward F. Haas, who explores the period since 1940, demonstrates the role of the federal government in stimulating rapid metropolitan growth around the southern rim of the continent and spotlights the question of race and the division between cities

and their suburbs as critical elements in the urban crises of the past thirty-six years. Each of the five essays is lucidly written, solidly documented, and cogently argued.

The book, of course, is not without its flaws. One is Goldfield's acceptance of a singlemindedly economic interpretation of Richard C. Wade's analysis of urban slavery. As Rabinowitz's piece implicitly acknowledges, Wade's assertion of the incompatibility of the peculiar institution and urbanization after 1820 emphasized the increasing difficulty of relying upon slavery as an instrument of racial discipline and social control, not the absence of versatility or adaptability in the slave system as an element in the urban labor force. Beyond that, however, the book is deeply divided against itself. The introductory essay, a survey of southern urban history by Brownell and Goldfield which takes the distinctiveness of the south as given, raises the problem of the relationship between urbanization and the idea of the South as a cultural region. The issue is never clearly resolved because the authors fail to recognize that the idea of the "ethnic" South is itself a relatively recent formulation with a history of its own, part of which is the story of how that view has influenced our perception and conceptualization of southern history. As the essays in this book suggest, however, a cultural definition of the South is difficult to sustain when the history of the area is scrutinized from the vantage point of the city. Indeed, it was the popularity of the idea of the South as a cultural region, a notion imbued with a strong agrarian bias, which distracted for so long professional historians from the task of analyzing urbanization in the southern half of the United States.

These reservations aside, this is a good and useful book. Anyone interested in the growth and diffusion of southern cities, boosterism, urban economic structure, municipal government and politics, and the question of race in an urban setting can benefit from a careful reading of these essays. The authors have all completed monographic studies in the periods they cover, and each has drawn broadly on the growing and heterogeneous literature in the field. Southern, American, and urban history are all the richer for the decision by the editors to disregard the vocational counsel of their parents.

ZANE L. MILLER
University of Cincinnati

KENNETH J. GRIEB. *The Latin American Policy of Warren G. Harding*. Fort Worth: Texas Christian University Press. 1976. Pp. viii, 223. \$5.00.

Most scholars would agree that the administration of Warren G. Harding marked a shift toward re-

trenchment in the nation's Mexican and Caribbean policies, and away from Wilsonian interventionism. Kenneth Grieb's new study describes this shift, maintaining that it also improved United States relations with the South American republics. Like other recent authors, Grieb also revises the traditionally negative view of Harding's presidency. He argues persuasively that Harding helped to set broad policy guidelines and acted as a force for diplomatic flexibility and openness, as against Secretary of State Charles Evans Hughes' more rigid and formal approach.

After a too-brief general introduction, the author devotes the balance of the book to chapters on Panama, Central America, the Dominican Republic, Haiti, Cuba, Mexico, the Tacna-Arica arbitration, and the Fifth Pan American Conference. Among Harding's main achievements, Grieb includes the ratification of the long-stalled treaty indemnifying Colombia for its loss of Panama, the arrangements for ending the occupation of the Dominican Republic, the reorganization of the Haitian occupation, a preliminary settlement of the Mexican oil dispute, and the agreement to arbitrate the Tacna-Arica controversy between Peru and Chile. In general, the chapters are short, clear summaries of stories largely known to scholars. The author's research in the papers of Harding, Hughes, and other participants, and in State Department records, shows up most importantly in the chapters on Central America and Mexico. In the former, Grieb convincingly clarifies the position of the United States toward Central American federation, showing that Washington ceased to support the federation movement only when it had clearly failed. The Mexican chapter is especially good on the background of the Bucareli Conference, perhaps reflecting Grieb's earlier Mexican work on *The United States and Huerta*.

While the book is a welcome contribution, it often lacks depth, and a fuller introduction would have enhanced the rest. Furthermore, the author's determination to do justice to the much-maligned Harding leads him at times into overcompensation. Harding is given the benefit of every doubt, and his policies in Haiti and Cuba, in particular, are portrayed in unduly affirmative tones. In short, this is a useful study of its subject, but hardly the final word.

DAVID HEALY
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NORMAN H. CLARK. *Deliver Us from Evil: An Interpretation of American Prohibition*. (Norton Essays in American History.) New York: W. W. Norton and Company. 1976. Pp. 246. \$10.95.

Deliver Us From Evil should deliver historians who have succumbed to the temptation of condemning American prohibitionists as narrowminded, bigoted, pietistic cranks. Some prohibitionists surely fit the stereotype; yet, as Norman H. Clark shows, it is erroneous to generalize from the excesses of zealots. Prohibition was a social reform integrally bound up with other reforms, all a response to perceptions of large and real problems produced by the social changes associated with the westward movement, urbanization, and industrialization.

Clark has two main objectives: to interpret the American prohibition movement in light of a developed body of monographic literature, and to explain how Americans after the 1920s came to see prohibition as antiprogressive. He satisfies both goals, partly by placing the American prohibition movement in a cross-cultural perspective. In the United States the 1810 per capita consumption of absolute alcohol was about 7 gallons; by 1850 temperance agitation had cut the figure to 2.10 (it was 2.61 in 1970). There was, thus, in the early nineteenth century a drink problem of major proportions. Drunkenness remained an object of reform for over a century as Americans continued to turn to ardent spirits for solace in a distended society, suppliers refined marketing techniques, and the highly competitive saloon business was ever more closely linked with vice and poverty.

Clark's thesis is that the prohibition movement was significant during a time when Americans were developing a "bourgeois interior"—a consciousness of individual as opposed to communal dignity. This value system was breaking down by the 1920s and was seen by intellectuals as a set of assumptions which retarded progress. As social thinkers and reformers sought new social and governmental institutions for socialization and social control—what Clark terms "the agency-society"—the use of the dangerous drug alcohol as an affront to self-dignity ceased to seem the compelling problem it had formerly posed. What was important in the new view of humankind was personal indulgence and individual freedom. This view was promoted not just in serious social thought but in popular culture as well. Under it, aided by the Great Depression and the Association Against the Prohibition Amendment, crumbled the intellectual and political foundations of prohibition. Historians living in the new "agency-society" often overlooked the ties in an earlier age between prohibition and other reforms.

No brief review can do justice to Clark's interpretation. Based not so much on new research as on a new conceptualization of social history, this is a revisionist account for all of us to ponder.

K. AUSTIN KERR
Ohio State University

FRANCIS RUSSELL. *The President Makers: From Mark Hanna to Joseph P. Kennedy*. Boston: Little, Brown and Company. 1976. Pp. vi, 407. \$12.50.

The President Makers is a depressing reminder of the gulf that exists between popular and professional historical writing. Francis Russell writes a crisp and serviceable prose, and he can skillfully describe characters and events. But his book adds almost nothing to our understanding of modern American politics. Nor does it bring to a larger audience anything of the work of the current generation of American political historians.

Russell's president makers and the presidents they are supposed to have made—Mark Hanna and William McKinley, Tom Platt and Theodore Roosevelt, TR and Taft, George Harvey and Woodrow Wilson, Harry Daugherty and Warren Harding, Frank Stearns and Calvin Coolidge, Louis Howe and Franklin Roosevelt, Joseph Kennedy and his son John—on first impression would seem to be a motley, unrelated crew. And so (at least in this book) they are.

Let us accept the author's contention that the president maker is a distinctive character in twentieth-century American politics. But what is the larger significance—what are the common (and distinguishing) characteristics—of this type? Do they reflect the increasing importance of a politics of money (Hanna and Joe Kennedy) and publicity (publisher Harvey and journalist Howe) over a politics of organization? But then what do we make of professional party men such as Platt, Stearns, and TR himself? Another fruitful topic of inquiry might have been the tension that so frequently arose between president maker and protégé after the great goal was attained. Does this relationship change in tone and tempo as the power of the presidency expands?

These and similar questions are never asked, let alone answered. Nor is any use made of modern work in American political history such as the Kleppner, Jensen, Marcus, or Rothman studies of the late nineteenth century, or the Hofstadter, Kolko, or Wiebe analyses of the Progressive period. What we have, in sum, is a sprightly series of unconnected vignettes, a lively example of a style of political history that is well into its anecdotal age.

MORTON KELLER
Brandeis University

NORMAN H. NIE *et al.* *The Changing American Voter*. (A Twentieth Century Fund Study.) Cambridge, Mass.: Harvard University Press. 1976. Pp. xx, 399. Cloth \$15.00, paper \$5.95.

The Changing American Voter presents wide-ranging commentary on political parties and voting behav-

ior in the United States over the four decades since the New Deal. It makes its greatest contribution, however, as a study of "issue voting," a subject which has received growing attention over the past half decade or so.

The general lines of the authors' argument can be easily summarized. A model of the American voter and the forces which shaped his voting decision was constructed by political scientists, notably those at the University of Michigan, during the late 1950s and early 1960s, drawing upon systematic survey research as a powerful and then relatively new tool. While this influential model has in many regards proved to be well founded, it has some notable weaknesses. In particular, those who developed it failed to appreciate the extent to which the electoral behavior they observed was a product of a very special style of politics which distinguished the United States during the 1950s. The model broke down rather severely in the 1960s, as the country moved beyond the relatively consensual politics of the Eisenhower years into the vastly more divisive conflict of the past decade. A growing salience of issues in the voting decision contributed to an increased attention to politics and to an enhanced sensitivity to ideology on the part of voters, and shattered the 1950s view of the electoral process, emphasizing as it did party loyalties and the relative impotence of the issue dimension.

As a study of issue voting and the changing environment which has nurtured it, *The Changing American Voter* has no peers. There is much more to this ambitious study, all of it imaginative, carefully analyzed, and clearly argued—and some of it certain to provoke a continuing scholarly debate. Clearly in the latter category and of special interest to historians is the argument of chapter 5 that the emerging Democratic majority of the New Deal era resulted largely from "generational replacement" rather than from "conversion." That is, large numbers of new voters came into the electorate in the 1920s and 1930s—as people reached voting age in the normal fashion and as members of a large pool of older Americans, mostly immigrants, voted for the first time—and became disproportionately Democratic. Working with the Michigan election study data of 1952 through 1972, in which respondents were asked not only their current party identification, but whether that identification had changed, and if it had, when, the authors reconstructed the partisan composition of the electorate for the 1920s, 30s, and 40s. On the basis of this analysis, they assert that a substantial plurality of Americans in the 1920s actually thought of themselves as Democrats. The Democrats lost the elections of the 1920s, even though they were in fact the majority party, be-

cause large numbers of their identifiers were recent immigrants who had not yet become part of the active electorate. When the immigrants were mobilized, beginning with the Smith election of 1928, they provided the Democrats with their New Deal majority. Remarkably little conversion occurred, the authors insist. Their data (p. 83) suggest that the proportion of the electorate identifying themselves as Democrats increased by only three percent between 1924 and 1936!

Other observers will remain inclined to an interpretation which accords greater "converting" powers to the Depression upheaval. My own analysis of extensive survey data, including Gallup studies from the 1930s and 1940s, suggests that the Great Depression did indeed "deauthorize" the Republican party. The number of people delivering Democratic votes increased in virtually all socioeconomic and age strata. Those more strongly committed to the Republicans did come back to the GOP as the Depression crisis eased, but large numbers of people, old voters as well as new, saw the Democrats as the more attractive party in the new political environment.

Such arguments will profit from further attention. None of this alters the fact, however, that *The Changing American Voter* is an important and provocative book. It deserves continuing scholarly attention.

EVERETT C. LADD, JR.
University of Connecticut

MICHAEL LEIGH. *Mobilizing Consent: Public Opinion and American Foreign Policy, 1937-1947*. Westport, Conn.: Greenwood Press, 1976. Pp. xvi, 187. \$14.50.

In October, 1935 George Gallup knocked and Pandora's box flew open. Before the first Gallup poll appeared historians had based their generalizations on their own sampling of public opinion. Increasingly, critics charged that the old method emphasized the elite and ignored the common man. Historians began to take more notice of polls; a few students began assuming a priori that mass opinion was more important than elite opinion in explaining historical questions. Other historians disagreed.

Given this climate of contention, all historians should applaud the effort by Michael Leigh to study the actual links between public opinion and three historical decisions: Franklin Roosevelt's Quarantine Speech in 1937; the establishment of the United Nations in 1945; and the adoption of the Truman Doctrine in 1947. Leigh challenges two conventional ideas about the role of public opinion: the traditional school, which assumes that mass opinion constrains leadership initiative; and the radical school, which argues that leaders

manipulate mass opinion to support planned policy. According to conventional historiography, writes Leigh, the Quarantine Speech led to a public reaction which limited Roosevelt's diplomatic options: a classic case for the traditionalists. The adoption of the Truman Plan, in contrast, revealed the Department of State busy manipulating public opinion: a classic case for the radicals.

Leigh, however, believes that neither the traditional nor the radical interpretation of public opinion linkage is satisfactory. Roosevelt mistook congressional opinion for public opinion in 1937. The radical critique of 1947 ignores the inhibitions policy-makers felt because of public opinion. Manipulation was a two-way street. Both the traditionalist and the radical have a simplistic view of how public opinion affects public policy.

The author has tackled a messy problem—how public opinion works in a given historical circumstance. We should commend him for clarifying the problem and laying to rest certain narrow notions. His attempt to merge a historical and social science approach prevented him from doing more. His conclusion, that “the form of participation [by public opinion] is determined by a complex set of factors,” ultimately falls back upon historical circumstances. The book rests upon the model approach of the social scientist, but the conclusion depends upon the historical analysis of circumstances. Yet Leigh fails to add very much to our historical knowledge. Dorothy Borg and Robert Divine have long since laid to rest the argument that Roosevelt drew back from interventionism because of negative public opinion. Neither does the author's treatment of the Truman Doctrine add much that is new.

GEORGE Q. FLYNN
Texas Tech University

RALPH B. LEVERING. *American Opinion and the Russian Alliance, 1939-1945*. Chapel Hill: University of North Carolina Press. 1976. Pp. xv, 262. \$17.95.

This well-written study is important for several reasons. First, it covers exhaustively an area that required monographic treatment. Further, it offers a wealth of information about the ways in which Americans viewed the Soviet Union during World War Two. Most significantly, it represents a valiant, if flawed, attempt to apply social science constructs to a traditional historical topic.

Ralph Levering sets out to accomplish four objectives in his reworked dissertation: to describe American opinions of Russia from 1939 through 1945, to analyze the process of opinion formation, to examine the role of leadership in that process, and to determine some of the preconditions that

led to the growth of anti-Russian sentiment in the early postwar years. He does a commendable job on his first and fourth objectives but falls somewhat short of the mark on objectives two and three.

Aside from looking at newspapers, magazines, public opinion polls, and nonfiction books, Levering turns up less orthodox sources including radio broadcasts, accounts of rallies, and college newspaper editorials. Although he roams far and wide through the media, he does not employ a formal sampling scheme; nor does he offer much hard evidence on the circulation or sales of his printed materials. Thus we must accept his assurances about the representative nature of his survey, assurances based upon his own immersion in the documentation.

Levering is interested primarily in opinions on diplomatic issues and not the more general attitudes about Russia and its political system that underlaid those opinions. His conclusions are sound if unsurprising. Most Americans disliked the Soviet Union before 1941; a majority came to admire Russians by 1943, became somewhat disenchanted in 1944 and early 1945, but were still prepared to maintain friendship as the war came to a close. Unfortunately, those opinions, attitudes, and predispositions were based upon unrealistic images of both Russia and world politics. Roosevelt is blamed here for failing to offer Americans a realistic view of the world. According to Levering, he was like a bartender who preferred to “make people happy, if only for a short time, than tell them the truth” (p. 185). Perhaps so, yet one wonders whether Roosevelt himself, through a combination of self-delusion and conviction, did not share some of the idealism and optimism he publicly exuded. And even if he had not, what would have happened to his bipartisan support had he suddenly told a woefully naive public about the seamier aspects of the Grand Alliance?

Throughout, Levering intelligently blends social scientific theories, poll data, and opinion sources. There are some problems, however. First, he allows the data to speak for themselves—in great abundance—and thus bogs the reader down in repetitive quotations from editorial material. Some quantitative summaries with fewer illustrations would have made for a more interesting narrative. In addition, he overuses the editorial page, one of the least read sections of newspapers and magazines. Lastly, he is not always clear whether cited material reflected or shaped opinion.

That there are things wrong with the book is, in part, clearly related to the ambitious strategy Levering adopts. His monograph is a useful contribution to the history of Soviet-American relations as well as a bold step toward the development of a

more sophisticated approach to the historical study of public opinion.

MELVIN SMALL
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LOUIS FISHER. *Presidential Spending Power*. Princeton: Princeton University Press. 1975. Pp. xiii, 345. Cloth \$12.50, paper \$3.45.

It is curious that presidential spending, which is a central feature and ultimately the engine of every administration, has been too little discussed and judged as an institutional process. Louis Fisher, who has served in the Government Division of the Congressional Research Service, writes here with great learning and familiarity on this elusive subject, and he has helped fill a vast gap in the writings on the presidency. Historians will long be in the debt of this political scientist.

The opening chapter is a particularly important one, for Fisher demonstrates beyond dispute that the presidential role in the making of the budget has been a significant one practically from the beginning of the presidential office. Moreover, he shows that since the Civil War the public has, in general, reposed greater trust in the president than in Congress as the protector of the public purse. But the chiefs have not always responded appropriately to this optimistic popular expectation. Through a variety of devices, including the reprogramming and impoundment of funds, the rescheduling of obligations, covert financing, and the making of unauthorized commitments, the chief executives have outmaneuvered Congress time and time again—or, interpreted more generously, they have introduced greater flexibility into the system of checks and balances than the Founding Fathers could have foreseen.

Fisher handles these powerful subjects in masterly fashion. They are not only fascinating in themselves, but they also raise trenchant questions about how the levers of presidential power have been operated in the past. Like the presidential power to appoint, the presidential power to spend deserves detailed study, administration by administration, in order to form a new basis for judging presidential performance.

Fisher's conclusions also deserve serious attention. He is committed to the virtue implied in Madison's declaration that "the legislative department alone has access to the pockets of the people." Still, he knows that Congress cannot ceaselessly play watchdog, and he knows that in the present shrunken world the president's strength depends substantially upon his ability to take discretionary steps, if necessary in a trice. Fisher is dogged. He writes in support of the solutions he advocates: "Visibility and accountability

are harsh standards for members of the legislative and executive branches who prefer doing business in less public ways. 'Government in the sunshine' is not in their interest." He is confident, however, that history is moving in the direction he prefers. He may be right. If he is, a new dawn will be upon us, and also a new presidency we cannot yet imagine.

HENRY F. GRAFF
Columbia University

MICHI WEGLYN. *Years of Infamy: The Untold Story of America's Concentration Camps*. With an introduction by JAMES A. MICHENER. New York: William Morrow. 1976. Pp. 351. \$10.95.

Notwithstanding a few notable exceptions like Roger Daniels, historians have left the crucial task of chronicling and interpreting the World War II evacuation experience of Japanese Americans to other cultural critics: anthropologists, psychologists, political scientists, sociologists, jurists, social philosophers, journalists, and artists. Moreover, aside from a handful of memoir accounts by former internees, the published literature on this social phenomenon has derived from those of non-Japanese ancestry. This lack of both a historical and an ethnic perspective accounts in large measure for its conflicting, yet complementary, tendencies toward either polemical cant or clinical detachment. Because the dynamic fusion of these two perspectives forms the very mainspring of *Years of Infamy*, it deserves recognition as a work of seminal importance.

Although avowedly not a professional historian, Michi Nishiura Weglyn has assuredly written a historical account of the "forgotten" or heretofore largely ignored dimensions of America's wartime imprisonment of more than 110,000 law-abiding Japanese Americans. The more fastidious practitioners of the historical craft justifiably will find fault with the author's tenuous chronological grounding, dilative use of choice documents, penchant for purple prose, astonished disclosure of human perfidy and bureaucratic malfeasance, and failure to achieve an overall symmetry. But for those whose fastidiousness does not vitiate their fairness, these flaws will seem trifling when set against Weglyn's abundant historiographical accomplishments. While *Years of Infamy* is theoretically and methodologically undistinguished, Weglyn has infused her inquiry with those time-honored qualities—sound research, compelling writing, strikingly original insight, contextual analysis, and what I would style passionate objectivity—that have traditionally distinguished first-rate historical works.

The author's vision springs from her ethnic per-

spective. In the case of Weglyn and most other Nisei, however, their initial reaction to the social fact of the evacuation differed too little from that of other Americans: "As a teen-age participant in this mass exodus I, like others, went along into confinement, trusting that our revered President in his great wisdom and discernment had found that the measure was in the best interest of our country . . . that this, under the circumstances, was the only way to prove our loyalty to a country which we loved." By reacting to their racist prewar exclusion from full participation in American society with an overcompensating Americanization, Nisei paid a steep psychological price: estrangement from their authentic experiences and needs as members of a victimized ethnic minority.

Even the flagrant abrogation of their citizenship rights represented by the evacuation failed to authenticate the ethnicity of many Nisei. And it is this (to coin a word) "unethnicated" element—the one hundred and ten percenters who cooperated with the authorities, signed loyalty oaths, and contributed to the war effort—that has preoccupied scholarly attention. In contrast, only scant and pejorative treatment has been accorded those ethnicated internees who frustrated the smooth operation of organized racism by their participation in camp revolts, their steadfast refusal to comply with bizarrely conceived loyalty questionnaires, and their willingness, if necessary, to renounce their citizenship. Instead of cavalierly dismissing this sector of the internee population as a small, subversive band of pro-Axis troublemakers, Weglyn shows that their number was considerable, their intentions often honorable, their plight representative—in an extreme way—of that of all internees, and their actions consonant with the American tradition of libertarian dissent. In so doing, she vindicates the honor of many wrongly impugned Japanese Americans, overcomes the debilitating effect of her own Americanization to remind us that "concentration camps and wholesale contempt for individual rights and lawful procedure are not the exclusive province of corrupt tyrannies and maniacal dictatorships," and forges a new interpretive framework for the next wave of scholars undertaking investigations into the evacuation experience.

ARTHUR A. HANSEN
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Fullerton*

E. B. POTTER. *Nimitz*. Annapolis: Naval Institute Press. 1976. Pp. xiii, 507. \$16.95.

Admiral Chester W. Nimitz (1885–1966) administered the naval war against Japan in the Pacific, and this is his story—superbly researched and

tastefully written by his authorized biographer. A clever and effective arranging of chapters—moving back and forth between the early war years and Nimitz's earlier life and career—makes for easy reading, while the vast reservoir of stories provided the author by Nimitz, his wife Catherine, and their children elevate the book far above the normal and often dreary military biography. The detail is fascinating (and excessive only in recounting V-J Day) and the good-humor most pleasing, as when Nimitz was mistakenly locked in a passenger train toilet by an unsuspecting porter and the Admiral counterattacked by imprisoning the distraught porter!

Potter succeeds admirably in describing the Pacific war from Nimitz's vantage point and is as fair as was his subject during those years in his treatment of all important and controversial figures—notably MacArthur, Spruance, Halsey, Towers, and Fletcher. Admiral E. J. King emerges as the real force behind decision-making in Pacific strategy, despite Potter's attempt to give Nimitz equal billing in those decisions. The only other instances of possible exaggeration are the author's giving Nimitz credit for co-inventing underway refueling as a lieutenant in 1917 and inventing the circular cruising formation as a commander at the Naval War College in 1922. Such assertions—and other tantalizing ones—could only be checked by the references, but Potter uses no specific footnotes, only a general listing by chapter in the back-matter. As for interpretations, this reviewer can only quarrel with Potter's defense of Spruance in the Battle of the Philippine Sea—a favorite debate among naval historians that reached an impasse many years ago.

The details of Nimitz's life make this biography most interesting to the knowledgeable student of World War II: Nimitz's meeting the victorious Admiral Togo in 1906, the cryptoanalysis work at Pearl Harbor, Nimitz's plane crash in 1942, and the Admiral's role in the firing of Admiral Louis Denfeld in 1949. He was a wonderful family man, a warm human being, and a brilliant administrator, but one who opposed the idea of women in uniform. No doubt, for reasons of space, many stories had to be left out, but it is strange that the author virtually ignores Nimitz's early career in submarines.

The charts are simple and attractive, the photographs well selected. Indeed, the author's perceptive connection of Tennyson's "Charge of the Light Brigade" to Nimitz's famous message to Halsey off Leyte Gulf is the perfect example of why this biography should be required reading for all students of the Pacific war—and for all aspiring military biographers.

CLARK G. REYNOLDS
U. S. Merchant Marine Academy

RICHARD E. DARILEK. *A Loyal Opposition in Time of War: The Republican Party and the Politics of Foreign Policy from Pearl Harbor to Yalta*. (Contributions in American History, number 49.) Westport, Conn.: Greenwood Press. 1976. Pp. x, 239. \$14.95.

Richard Darilek's brief monograph reminds us that post-World War II bipartisanship in foreign policy developed rapidly between Pearl Harbor and Yalta. Darilek's purpose is to discover how and why this enduring phenomenon took place.

His findings are clearly stated and in no way surprising. Republican leaders, he argues, adopted the posture of bipartisanship in order to preserve, not to stifle, opposition to Franklin D. Roosevelt's policies. Leading Republican spokesmen—Arthur Vandenberg included—never experienced “basic changes” in ideology or personal conviction; there were no great “conversions” to internationalism. Rather, “party leaders came to see it [bipartisanship] as a strategic solution to some basically political problems.” Using James MacGregor Burns' model of “four-party government” in America, Darilek contends that internal divisions between the GOP's congressional and “presidential” wings constituted the chief “political problem” for the party at the time. Darilek's book thus becomes the story of how these two wings—Wendell Willkie, Thomas Dewey, and John Foster Dulles on the one side, Robert Taft and more “isolationist” congressmen on the other—attempted to reconcile their internal differences. Bipartisanship, Darilek argues, helped them succeed in doing so. The result was a muting of partisan battles during the war, and a diminution—probably unfortunate in the long run—in congressional independence and constructive criticism of foreign policies hatched in the executive branch.

Darilek disarmingly concedes the narrowness of his study, which focuses only on Republican leaders, for less than four years. His conclusions are not likely to startle anyone who has worked in the field. His argument is nonetheless clear and well researched.

JAMES T. PATTERSON
Brown University

J. SAMUEL WALKER. *Henry A. Wallace and American Foreign Policy*. (Contributions in American History, number 50.) Westport, Conn.: Greenwood Press. 1976. Pp. x, 224. \$13.95.

As Cold War tensions between the United States and the Soviet Union have been redefined and relaxed in recent years in response to a radically changed world situation, the debate among American scholars over the origins and development of the cold war has broadened and deepened. One aspect of this debate has been the rediscovery of

Henry A. Wallace, the leading opponent in the United States of the Truman cold war policies in the late 1940s, with the subsequent emergence of a scholarly literature that both clarifies and analyzes Wallace's role in national and international affairs. In this well-written and well-researched monograph, J. Samuel Walker has made a useful contribution to this literature, adding to the previous studies of E. L. and F. H. Schapsmeier, Alonzo Hamby, John M. Blum, and more recently, Richard Walton.

Although those who are familiar with Wallace scholarship will find no surprises in Walker's study, the author sticks close to the documents, integrates them with the secondary literature, and makes good use of the very recently opened Wallace diaries and oral history reminiscences to present a sympathetic description of Wallace's developing perceptions of foreign policy. Although Walker's chapters are occasionally too short for the topics that he covers, his monograph complements the existing literature on a wide range of foreign policy questions, especially for the period to 1945. As an interesting sidelight that displays his abilities to do careful empirical work, Walker presents the best documented account to date of Wallace's involvement with Nicholas Roerich and the subsequent “Guru letters” controversy that developed from their relationship.

The monograph, however, is not without flaws. First of all, Walker is most sketchy in dealing with the important post-1946 period, when both Wallace's government service and diaries were discontinued. Walker also fails to pay adequate attention to the domestic political climate and its effects on Wallace's thought and actions. While Walker does use interviews, these are mostly with Wallace's friends from the USDA rather than with political and governmental figures who might have been more valuable for the monograph. Finally, as I see it, Walker, rather like Wallace himself, fails to address the broader questions of imperialism and monopoly capitalism in the modern era, and their deleterious effects on liberal aspirations. But, even with these criticisms (and the last are, of course, wholly a matter of general outlook), J. Samuel Walker has produced a solid little monograph that contributes significantly to the scholarship on Henry Wallace.

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RICHARD J. WALTON. *Henry Wallace, Harry Truman, and the Cold War*. New York: Viking Press. 1976. Pp. x, 388. \$12.95.

Although the authors of a number of recent books, articles, and dissertations deal sympathetically

with Henry Wallace's attempt to challenge the assumptions behind American foreign policy in the early cold war era, none accept his views as uncritically or embrace his position as enthusiastically as Richard J. Walton in *Henry Wallace, Harry Truman, and the Cold War*. After tracing Wallace's deepening disillusionment with the Truman administration's foreign policies from 1945 through 1947, Walton provides a detailed analysis of the Progressive candidate's ill-fated 1948 presidential campaign. He denounces Truman, cold war liberals, organized labor, and the press for smearing Wallace as a Communist dupe and he concludes that in assessing the postwar international situation, "Henry Wallace was essentially right and Harry Truman was essentially and tragically wrong" (p. 355).

In his effort to vindicate Wallace, Walton overlooks or minimizes the weaknesses of his subject's stance. Although he argues convincingly that Wallace was a victim of the cold war consensus, he fails to discuss the extent to which the futility of the 1948 campaign stemmed from Wallace's sometimes untenable positions and often overwrought rhetoric. The moderate and sensible criticisms of U.S. diplomacy that Wallace advanced in 1946 and early 1947 became shrill and dogmatic attacks by 1948, and his frequently voiced fear of domestic fascism was no more rational than the country's growing obsession with Communism. Like Wallace in 1948, Walton undermines his own credibility by making extravagant statements. For example, he asserts but offers no substantive evidence that the Marshall Plan was "a mistake of gargantuan proportion" (p. 199) and that Truman seemingly "did not genuinely want good relations with Russia" (p. 220).

Walton's book suffers from the meagerness of his research as well as from his unabashedly partisan analysis. Still, he tells an important and absorbing story, and one can hope that he reaches the general audience at which he aims. Walton will have provided a useful service if his book acts as an antidote to the Truman cult and helps counterbalance the distortions and inaccuracies perpetrated by those writers who have canonized the image of Harry Truman in the popular mind.

J. SAMUEL WALKER
Office of Educational Programs,
National Archives

Foreign Relations of the United States, 1948. Volume 1, part 2. *General; The United Nations*. (Department of State Publication 8849.) Washington, D.C.: Government Printing Office. 1976. Pp. xii, 507-1016, xx. \$8.50.

This volume covers three crucial topics: national security, atomic energy, and international trade. Civilian and military officials in 1948 saw Russia determined to dominate the world. The Policy Planning Staff under George F. Kennan, partly at the request of Defense Secretary Forrestal, worked closely with the National Security Council to develop a strategy for countering the Communist menace. The military regarded American power as insufficient to handle any direct crisis and lobbied for increased funds.

The State Department placed major emphasis on the European Recovery Program (ERP) in resisting Russia. Most analysts ruled out direct attack by Russia, holding that America's nuclear monopoly provided a key deterrent. As to atomic warfare, Edmund Gullion, special assistant to Undersecretary Lovett, reported, "So far as I know little thought has been given to this problem in this Department" (p. 571). While this deficiency was being remedied, reliance on the UN faded. Kennan attacked it as a misleading panacea in the public mind, which mired policy "in meshes of a sterile and cumbersome international parliamentarianism . . ." that had led to "a great dispersal of our effort" (pp. 528, 529).

The U.S. secured a *modus vivendi* with Britain and Canada which gave America freedom in using the A-bomb and a larger share of nuclear raw materials. The U.S. unsuccessfully tried to persuade Britain to rely on using American bombs and encountered acrimony with London over British insistence on providing Norway with some nuclear technology. The Americans carefully sought to keep atomic policy separate from ERP negotiations to avoid Soviet charges that the U.S. sought an atomic monopoly. Washington employed bilateral talks to lock up nuclear raw materials in other nations and prevent the proliferation of atomic developments.

At the Havana Conference to establish an International Trade Organization (ITO), the State Department tried to implement principles set forth in the General Agreement on Tariffs and Trade. The Americans viewed ITO as a key element in moving toward economic multilateralism which would provide a workable vehicle in relations between industrial nations and third world countries. Chief U.S. negotiator Will Clayton encountered severe opposition from Latin Americans who feared continuing trade discrimination and former European Allies who opposed most favored nation status for occupied Germany—something the Americans wanted to facilitate the integration of Germany into the postwar European recovery. The volume concludes with a short section on Antarctica.

It is disappointing that the editors have included such limited representation in the area of national

security, and it is to be hoped that future volumes will reverse this situation. In all, this volume is where historians should begin in understanding foreign policy for 1948.

THOMAS M. CAMPBELL
Florida State University

Foreign Relations of the United States, 1948. Volume 3, Western Europe. (Department of State Publication 8779.) Washington, D.C.: Government Printing Office, 1974. Pp. xiii, 1165. \$12.90.

Foreign Relations of the United States, 1949. Volume 4, Western Europe. (Department of State Publication 8791.) Washington, D.C.: Government Printing Office, 1975. Pp. x, 878. \$11.15.

The materials contained in these volumes have already been more or less systematically mined by historians concerned with various aspects of the cold war and thus offer few surprises. They continue, however, the tradition of the series as an accurate barometer of the international climate, which can be read by examining the size and scope of the various volumes. Not surprisingly, the 1948 volume is both the larger and the more interesting. Half of it is devoted to matters relating to Bevin's proposal for a Western European Union, to the organization of the European Recovery Program, to the formation of NATO, and to the continuing controversy over the Free Territory of Trieste. A significant amount of space is also given to direct relations with France, the United Kingdom, and Portugal, and to the push for liberalization of the Franco regime in Spain and its reintegration into the international community. Nearly a quarter of the volume deals with American fears about and actions concerning the Italian elections. In the considerably briefer volume for 1949, Italy disappears as an area of serious concern, and most of the other matters are simply continued in the direction they had taken by the end of the previous year.

Perhaps the most complete and informative sections of these volumes deal with the planning and discussion preceding the formation of NATO. The key area of debate was not the basic concept, but the role of the Scandinavian countries and of Italy. The influence of Norway, and of its foreign minister, Halvard Lange, is clearly set forth in the documents, as is the somewhat tortuous route by which Italy was finally included, despite the continuing opposition of both the president and the Policy Planning Staff. Truman apparently opposed Italy's participation as late as February 28, 1949 (1949, p. 125).

Most illuminating in retrospect is the section dealing with the Italian elections of 1948, which

reflects accurately at least one part of the cold war mentality by clearly setting forth the State Department's near-panic over the possibility of a Communist victory. Not only were frantic and sometimes harebrained schemes for influencing the outcome of that election set in motion, but George Kennan went so far as to suggest that the Italian government outlaw the Communist party and take strong action against it. "Communists would presumably reply with civil war," he concluded, "which would give us grounds for the reoccupation of Foggia fields or any other facilities we might wish. This would admittedly result in much violence and probably a military division of Italy; but we are getting close to the deadline and I think it might well be preferable to a bloodless election victory . . . which would give the Communists the entire peninsula" (1948, p. 849).

Among other matters of interest is documentation of the American attempt to maintain "a centrist or middle-of-the-road coalition" in power in France in order to avoid both the Communist Scylla and the Charybdis of Charles De Gaulle. The State Department had little liking for De Gaulle's alleged "stubbornness, disregard for economic, financial and social considerations, mysticism, and strong but hazy ideas of how to govern" (1948, p. 653). Also of interest is the maneuvering in Trieste, which constitutes an intriguing counterpoint to Soviet activities in eastern Europe. In this case it was the United States and the United Kingdom who stymied an agreed-upon settlement by preventing the appointment of a governor for the Free Territory on the correct assumption that continuance of the status quo offered the best chance for frustrating Yugoslav designs and allowing the city to drift back into Italian hands. Noteworthy too, in light of subsequent developments, is the discussion by David K. Bruce, then ambassador to France, of what he called "the Indochina problem" on October 22, 1949. "Of course," he insisted, "we were against colonialism because it didn't work and couldn't work, and for nationalism because it was the strongest force in Southeast Asia. But could we afford to be purists and perfectionists?" (1949, p. 495). Could we afford not to be?

The editing of these volumes conforms to the usual high standards of the series. One might wish for more summaries of "documents not printed" and a clearer indication of what collections of materials were not included. The cross references to other volumes in the series are helpful, but they are regrettably, perhaps unavoidably, insufficient to enable the reader to keep track of the full flow of events.

MANFRED JONAS
Union College

Foreign Relations of the United States, 1948. Volume 6, *The Far East and Australasia*. (Department of State Publication 8681.) Washington: Government Printing Office. 1974. Pp. x, 1379. \$14.40.

The Far East and Australasia is partially a misnomer for this volume in the annual *Foreign Relations* series. There are no documents on China or Siam (Thailand) and only a very limited number on French Indochina, the Philippines, and Australasia. Nearly all the material is on the Netherlands East Indies (Indonesia), Japan, and Korea.

For Indonesia, 1948 was a significant year as the Republic of Indonesia fought to obtain independence from the Netherlands. The 568 pages of extensive documents delineate America's efforts to obtain a negotiated settlement through independent action and as Chairman of the Good Offices Committee (GOC) of the United Nations. The United States tried to persuade the Dutch to compromise and to accept the United States of Indonesia as a sovereign power in a Dutch Union. Negotiations began in January 1948 with the signing of an agreement on board the USS *Renville*, but the year ended in failure as the Dutch, ignoring pleas by the United States and the United Nations, initiated a second police action in December, capturing most of the Republican leadership. The United States protested and applied pressure on the Netherlands by terminating aid given to the Dutch for Indonesia, although the State Department was very careful not to threaten withdrawal of European Recovery Plan (ERP) funds to the Netherlands. Though the State Department rejected all suggestions of economic sanctions or the breaking off of diplomatic relations with the Netherlands, it did threaten to withdraw from the GOC and grant diplomatic recognition to the Republic if the Dutch continued to refuse to negotiate. No success was obtained before the end of the year and the story will be continued in the 1949 volumes of the *Foreign Relations* series.

The 431 pages of documents on Japan, including excellent reports by William J. Sebald, acting political adviser in Japan, and George F. Kennan, director of the Policy Planning Staff, who held discussions with General MacArthur in Japan in March 1948, were primarily concerned with preparations for a peace treaty, security arrangements for the postoccupation period, the reparation issue, and relaxation of Occupation controls. Throughout there was an underlying theme, particularly in the reports by W. Walton Butterworth, director of the Office of Far Eastern Affairs, of the State Department's desire to curtail the power of General MacArthur by firmer direction from Washington as well as by the appointment of an American ambassador and a civilian deputy for the Supreme Allied Commander (SCAP).

Most of the documents in the 266 pages on Korea were on implementation of the United Nations General Assembly Resolution of November 14, 1947, which called for national elections in Korea. The United Nations Commission was immediately attacked by the Soviet Union, which refused to allow it to function in North Korea and proclaimed the establishment of the Democratic Republic of Korea in February 1948. State Department policy was to push for elections in the south and for withdrawal of the 20,000 American troops based on a flexible timetable which considered the strength and training of the South Korean defense forces although the United States Army wanted to withdraw by December 31, 1948. The State Department, the Army, and the National Security Council agreed that "the U. S. should not become so irrevocably involved in the Korean situation that any action by any faction in Korea or by any other power in Korea could be considered a *casus belli* for the U.S." (1169). Subsequent to the May 10th elections, the State Department was intimately involved with the new South Korean government of Syngman Rhee. A number of problems existed and were discussed in the documents including diplomatic recognition, role of the United Nations, withdrawal of American forces, recall of Lt. General John R. Hodges, and Soviet reaction to the new Rhee government.

The documents in the annual volumes of the *Foreign Relations of the United States* are primarily State Department records with some reports from other agencies such as the National Security Council. These documents are of vital interest to scholars although usually there are no new major startling revelations. All the *Foreign Relations* volumes have excellent explanatory footnotes and good indexes.

KENNETH RAY YOUNG

Western Connecticut State College

Foreign Relations of the United States, 1949. Volume 1, *National Security Affairs, Foreign Economic Policy*. (Department of State Publication 8850.) Washington, D.C.: Government Printing Office. 1976. Pp. xxv, 836. \$11.00.

This is one of nine volumes scheduled for the year 1949 in the excellent series of officially published documents on American foreign relations. Arranged chronologically by subject and issue, this volume maintains the series' tradition in its technical excellence and in its value as a teaching aid and a research tool. Students and scholars will find here a large, readily available collection of documents, remarkable for their coverage, fullness, and modest price. Researchers will also find explanatory footnotes, reference notes, editorial notes, record group identifications, file numbers,

and a host of other aids that are extremely valuable to the expert in search of additional detail and documentation.

The major collections printed in this volume deal with national security and foreign economic policy. Under national security one finds material on atomic energy, the regulation and control of conventional armaments, strategic stockpiling, the American search for military bases and air transit rights, and U.S. military assistance under the Mutual Defense Assistance Act of 1949. One also finds a number of studies on the possibility and probability of war in the future. There are documents listing our potential enemies and our potential friends, and identifying those about whom there was some doubt—all of which reminded this reviewer of the black, white, and grey lists that Americans used to facilitate denazification in Germany after the war. National Security Council and Policy Planning Staff papers appear often in this section.

The collection on foreign economic policy contains a large number of documents on the continuing negotiations under the General Agreement on Tariffs and Trade (GATT) and lesser numbers on the International Monetary Fund and the origins of the U.S. foreign aid program that developed out of the fourth point of President Truman's inaugural address on January 20, 1949. Interesting for those who see American policy as a reflection of clear objectives and long-range plans is the fact that the Point Four program took shape in the State Department only after the president announced it publicly, and that at least two months went by before the State Department produced a paper on the "Objectives and Nature of the Point IV Program" for Truman (pp. 774-83).

In addition to the two major collections, there are brief sections on State Department reorganization, civil aviation policies, and U.S. policy regarding the Antarctic.

JOHN GIMBEL
Humboldt State University

DAVID S. MCLELLAN. *Dean Acheson: The State Department Years*. New York: Dodd, Mead and Company. 1976. Pp. xii, 466. \$17.50.

This new assessment of Dean G. Acheson and his influence on American foreign policy begins with a biographical treatment of family background, youth, education, professional experience as a lawyer, and government service under Franklin D. Roosevelt. But it deals with these portions of Acheson's life as prologue to the years served as under-secretary and secretary of state. The book ends with an epilogue or brief commentary on the period after he left office until his death in 1971. As a history of recent foreign policy with the focus on

one of its most important shapers, this study has many virtues. It is soundly and intelligently researched, grounded on unpublished materials, interviews with some key and minor figures in the events studied, and the most recent published literature. Throughout the author shows familiarity with the nuances of left and right views on the cold war and on Acheson himself. Furthermore, the writing is usually clear, often graceful, and moves well. Those who share the author's premises will also accept his evaluations and general interpretation as praiseworthy.

Those who differ will probably consider David McLellan's unabashed commitment to Acheson and cold war policies as the book's major flaw. McLellan not only accepts Acheson's cold war stance as just and proper, but also argues with or attacks critics, especially revisionists of the left. He maintains, for example, that Acheson's view of the cold war "was dominated by the deadly logic of the struggle." At another point he finds it hard to see "how revisionists can argue that Soviet behavior was dictated by Acheson's aggressive diplomacy." When dealing with Asian policy, however, McLellan is truly critical of Acheson. He argues that in Asian matters the secretary of state lacked the intuitive mastery that he had of diplomacy in European affairs.

McLellan, it seems, exaggerates Acheson's intellectual qualities, his accomplishments, and the power he wielded. Harry S. Truman, the source of the secretary's power, appears to lurk unimportantly in the shadows. The problem lies, in part at least, in the nature of the book. With some exceptions those who write biographies become partisans of their subjects, and McLellan is no exception. He appears enamored of Acheson, the man he considers the architect of the nation's most successful postwar foreign policies, an echo of Acheson's own less-than-modest retrospective view of his role.

Undeniably Acheson held office during years crowded with momentous developments, and he, more than almost any modern secretary of state, had decisive influence in shaping and conducting foreign policy. Yet, as Gaddis Smith points out in his more critical study, Acheson was a man of limited vision. He saw only one side of the cold war, which he identified as both his own and the nation's. He believed in the moral and political ascendancy of Americans over other peoples, and acted accordingly. This attitude, or similar ones, motivated many in the establishment that gave the nation cold war intransigence, the Korean War, the Vietnam War, and the imperial presidency.

As for personal qualities, Acheson had courage. He opposed McCarthyism and stood up for elected civilian control over military arrogance, as in the Douglas MacArthur crisis. Acheson himself

was also arrogant, condescending toward those he considered social or intellectual inferiors, and intolerant of dissent. In a democracy he was an elitist who held egalitarianism in contempt. He thought it proper for power, immense power in foreign relations, to be concentrated in the hands of a few, and hence he approved and defended presidential wars regardless of their debatable legality. He believed that force, or military power, was what really counted in foreign policy and hence ridiculed collective security. Ironically, during the Korean War he portrayed himself as a champion of that principle.

Whether or not Acheson's policies were sound, he considered them to be in the national interest. He was, moreover, an effective and powerful secretary of state, certainly not a Dean Rusk or William Rogers. With proper deference to his chief, he was in constant command of foreign policy, and therefore some, such as Henry Kissinger, consider him "the greatest Secretary of State of the 20th century." This book tells us how and why such a judgment is possible.

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ROBERT A. HART. *The Eccentric Tradition: American Diplomacy in the Far East*. New York: Charles Scribner's Sons. 1976. Pp. 277. Cloth \$10.95, paper \$4.95.

Robert Hart, a journalist turned academic, has dished up a breezy, anecdotal interpretative history of United States relations with Asia. His recipe comes right out of George Kennan's "realist" critique of American foreign policy. The chief ingredients are a description of the pendulum-like swings in American policy between overinvolvement and underinvolvement in Asian affairs (a tendency Hart deplores) and an exploration of the popular emotions and ideals which (Hart feels) account for our erratic national behavior in Asia. In Hart's scheme of things, interwar isolationism comes in for as much criticism as the various Asian crusades from McKinley's Philippines folly down to the Indochina war; praise goes to Theodore Roosevelt, Franklin Roosevelt, Harry Truman, and the Nixon-Kissinger team for achieving a modicum of moderation despite public intemperance on Asian questions.

Taken on its own terms, Hart's "eccentric tradition" is not an altogether convincing explanation of the peculiarities of American policy toward Asia. The essential point that a fickle and uninformed public shaped crucial policy decisions is not argued with the rigor and care necessary to carry along readers with reservations about the realist interpretation. Hart's prescription for a bet-

ter policy—put diplomacy in the hands of professionals insulated from a mercurial public—still leaves us with the problem of deciding which professionals to trust. Incongruously enough, Hart expresses his technocratic faith in a volume peopled with as many realistic amateurs as misguided professionals.

Viewed in an international context, *The Eccentric Tradition* itself falls within what increasingly seems an eccentric historical tradition of discussing American-East Asian relations from an essentially American point of view. Confronted by a daunting array of countries and cultures, Hart has understandably decided to emphasize the American perspective. But the inadvertent and ironic result is that a curious and dated picture of Asia and Asians repeatedly creeps into the account. Thus, the Chinese are "philosophic" (p. 12) as they face the West in the nineteenth century; the Perry expedition jolts an inert Tokugawa Japan into an era of change; the Chinese play only a passive role in Taft's dollar diplomacy; rampant militarism accounts for Japanese aggression in the 1930s; the Chinese Communists are inexplicably hostile to the United States after World War II; the Korean War has no significant Korean dimension; and North Vietnam perplexingly stands in the way of Nixon's search for an honorable peace in Indochina. The cumulative impact of these oversights and stereotypes is to perpetuate that very tendency to see Asia in American terms that Hart himself decries and wishes to inoculate his readers against in the name of realism.

MICHAEL H. HUNT
Yale University

STANLEY D. BACHRACK. *The Committee of One Million: "China Lobby" Politics, 1953-1971*. New York: Columbia University Press. 1976. Pp. xi, 371. \$14.95.

This is a well-researched and lucidly written study of the major pressure group seeking to influence American China policy. Making excellent use of available sources, among them the papers of Marvin Liebman who for sixteen years served as secretary of the Committee of One Million, Stanley Bachrack provides a wealth of useful and interesting information.

In the first fifty pages he discusses the overarching ideology that came to dominate U.S. thinking about China prior to the Communist victory in 1949, and he defines the "China Lobby." He then suggests that while the pro-Nationalist sentiment extant in the United States during the Korean War might well have resulted in the eventual organizing of a formal pressure group, the actual establishment of the Committee For One

Million—as the organization was first called—may have occurred at the instance of the C.I.A.

In the remainder of the work Bachrack treats the China policy of each American administration from Eisenhower to Nixon, in each case describing the efforts of the Committee in discharging its central purpose, i.e., preventing the recognition of the People's Republic or its admittance into the United Nations. On the latter, the group proved steadfast and single-minded to the bitter end, that is until about 1971.

The consistency of the Committee is explained largely by its leadership, and the book contains a good deal of material on Marvin Liebman and Walter Judd. It also includes accounts of intra-Committee debates, the secretary's fund-raising activities, and contact between the leadership and Chinese Nationalist diplomats. The interaction of the Committee with Congress, indeed congressional membership in it, receives considerable attention as well.

Without in any way derogating the author's efforts, one might make critical observations about several points. The work is more descriptive than explanatory, and one wonders if Bachrack could not have offered a more penetrating analysis of the Committee's inflexibility. For instance, Walter Judd's China policy assessment in the late forties, the record demonstrates, was rooted partially in *Realpolitik*. Why Judd would remain unmoved by changed circumstances arising out of the Sino-Soviet split even into the 1970s is a question that deserves study. Another question, and one that in fairness the author cannot answer because of a dearth of documents, is whether the Committee had any appreciable effect on policy. Bachrack intelligently speculates that it did, to some extent, but the question remains open.

RUSSELL D. BUHITE
University of Oklahoma

ROBERT F. BYRNES. *Soviet-American Exchanges, 1958-1975*. Bloomington: Indiana University Press. 1976. Pp. xii, 275. \$10.00.

Cultural, scientific, and educational exchanges with the USSR have been so widely publicized as a recent by-product of detente that many Americans are unaware that US-USSR scholar exchanges have been going on for almost twenty years. For much of the period since 1958 these small—and always difficult—programs have provided a thin lifeline of communication during good times and bad, and have contributed significantly to American understanding of the Russian and Soviet past and present.

Robert F. Byrnes was present at the founding and served as chairman of the Inter-University

Committee on Travel Grants (IUCTG) from 1960 to 1969, when its functions were transferred to the newly created International Research and Exchanges Board (IREX). His book is a history and evaluation of the IUCTG period (and to some extent of the IREX period) as well as a commentary on the urgent need for—and peculiar dangers of—scholarly communication between an open society and a closed one.

Byrnes recounts the painstaking efforts of the American academic community, especially the Slavists and historians, to organize and administer programs with a partner which often as not seemed more concerned with hindering than promoting cooperation, and recalls the pitfalls and triumphs as the Americans learned to cope with political and educational bureaucracies that were *terra incognita* after a long period of Stalinist isolation from Western contacts. At the same time, our universities had to devise ways to prevent the program from becoming a political and diplomatic football in our own government and to safeguard its integrity in an atmosphere of lingering McCarthyism. This effort succeeded, and Byrnes himself played a major role, though he is too modest to say so. (Alas, the lesson that university independence is as vital to our government as it is to the universities is one that has to be learned by each new generation of Washington officials, but the IUCTG provided a powerful precedent.)

While the book is at its best in chronicling the American institutional response to a completely new and unfamiliar form of international contact, it is less successful in assessing what the Soviet partner is really like and misses many of the important changes which have taken place in Soviet attitudes and practices in recent years. In particular, the vision of an essentially adversarial relationship with a monolithic opponent was never entirely accurate and is less so now. Strategies based on such an assumption fail to take into account the existence in the Soviet Union of conflicting (although seldom publicly expressed) views in rather high places about how to cope with the considerable impact of the American academic presence in the USSR and of the experiences of returning Soviets who have spent some time here. These conflicts and differences create a constantly shifting set of openings through which to promote and encourage a freer exchange of people and ideas—which is what the exchanges are about.

The Soviets are increasingly aware that they cannot survive in the modern world without participating to some degree in the international division of scholarly and scientific labor, and their grudging acceptance of this reality (despite their continuing efforts to contain its political and cultural implications) gives ground for hope. Byrnes

does not exaggerate the difficulties and frustrations of the game, but it is nevertheless one to which the United States brings many important advantages of scholarly and scientific resources, as well as the institutional and organizational strengths which Byrnes and his colleagues helped to create.

ALLEN KASSOF
*International Research
and Exchanges Board,
New York City*

FREDERICK S. ALLEN *et al.* *The University of Colorado, 1876-1976.* (A Centennial Publication of the University of Colorado.) New York: Harcourt Brace Jovanovich. 1976. Pp. xii, 319. \$12.95.

The most recent president to depart from the University of Colorado was relieved of his duties by the regents in 1974, a year after a faculty vote of "no confidence." But as this well-researched work by five historians at the University demonstrates, few previous administrations had gone without serious protest of some kind. And despite every difficulty the enrollment and physical plant have expanded, with campuses now at Denver and Colorado Springs as well as Boulder. At the same time the graduate program has risen to regional and even national prominence, especially in the natural sciences. Almost to crown such success, Colorado has become a top-ranked football power.

Although Allen and his colleagues try to survey university life generally, their principal focus remains on the administrative record. The six main chapters begin and end with presidential changes. The only persons to emerge with any clarity are chief executives. And on them seems to settle most of the praise or blame for the working of the whole educational enterprise. Such may be typical of the American state university; it was surely so at Colorado.

Particularly evident there also were the conservative-liberal conflicts after World War II over left-wing associations, loyalty oaths, racial discrimination, intellectual elitism, and antiwar demonstrations. Clearly on the liberal side themselves, the authors of the three post-1939 chapters (Mitterling, Scamehorn, Allen) nonetheless examine these controversies carefully and dispassionately, making use of interviews and regents' minutes as well as administrative files. Given the partisan division that often marked these disputes, it will be surprising if the candor of this centennial history does not provoke fresh Republican criticism.

Allen, a historian of modern Europe, not only composed prologue, final chapter, and epilogue but served capably also as editor. His associates were not really specialists in the periods they cov-

ered either, which perhaps contributed to the volume's cohesion in approach, organization, and style. For a project carried out in less than two years amid regular teaching assignments, this team effort achieved remarkable results; it is only regrettable that they had to compress and catalogue so much, and that they did not explore the university's effects upon other educational institutions in Colorado.

G. WALLACE CHESSMAN
Denison University

GODFREY HODGSON. *America in Our Time.* Garden City, N.Y.: Doubleday and Company. 1976. Pp. xii, 564. \$12.95.

This book is essentially an extended essay on those aspects of the social and intellectual history of the sixties that most attracted the attention of its author, a noted British journalist. As one might expect, it is quite good in its discussion of the news media. It is equally strong in its coverage of the discontents of the reform-oriented upper-middle class, especially the young.

The volume's unifying theme is the collapse of "the ideology of the liberal consensus" that had emerged from the forties and fifties. One basis of this consensus was a renewed faith in American capitalism, specifically its ability to generate an economic growth that would lift the working class into the middle class while also providing the resources to attack social problems without class conflict. The other basis was a belief that the United States was destined to extend the blessings of its system around the world and to defend the less fortunate from the evils of Communism.

Godfrey Hodgson appears to be a left-liberal who stops short of socialism and is unable to take the American New Left very seriously; but he is equally put off by the optimism of the liberal establishment. He is outraged by the assertion that the working class has joined the middle class, but his attack upon the claim is rather ineffective. One feels that he has had little real contact with the American working class in all its variety; he fails to discuss its inner differences and various aspirations with a tenth of the perception or space that he devotes to the revolt of the privileged. He simply refuses to face the widespread blue-collar acceptance of bourgeois values. Instead, in quaintly European fashion, he confuses the upper strata of middle-class America with the class as a whole and tells us that, "Steamfitters don't live on the same block as surgeons."

In the same vein, the author rejects growth economics in favor of a redistributive political economy; but he is about as fatuously optimistic as the establishment liberals in his failure to address two

critical questions: What would be the economic consequences of a no-growth economy? How much would be redistributed in what fashion from whom to whom?

Whatever its limitations, this is a readable and often rewarding book. It does not match William O'Neill's *Coming Apart* as a comprehensive narrative of the sixties, but it merits the attention of all historians interested in that decade.

ALONZO L. HAMBY
Ohio University

JEROME L. RODNITZKY. *Minstrels of the Dawn: The Folk-Protest Singer as a Cultural Hero*. Chicago: Nelson-Hall. 1976. Pp. xx, 192. Cloth \$9.95, paper \$4.95.

Remember for a moment the dawn of the 1960s when the folk-protest singers were going to lead the world into a new era of love, peace, brotherhood, and sisterhood? This well-argued book places the cultural contributions of Woody Guthrie, "the father of the now-generation," Joan Baez, "a pacifist St. Joan," Phil Ochs, "a minstrel's search for martyrdom," and Bob Dylan, "beyond left and right," in proper societal and historical perspective. These and other "minstrels of the dawn" were important symbols and heroes for large numbers of 1960s youth, a group which some historians label the "generation of protest."

Important conceptual chapters discuss the evolution of the American protest song, protest music as religious experience, and popular music as a radical influence. Jerome L. Rodnitzky judiciously argues that the best protest song writers have been a "cleansing force" in our society supplying "examples of conscience and principle" to a contemporary society that was often lacking in suitable examples. He also uses good historical judgment in assessing the possible limited importance of the folk songs and singers themselves yet stating their value as symptoms of a "dramatic change in the social awareness of the younger generation." It is carefully noted, though, that since the generation that grew on these symbols has not yet reached full maturity or levels of power, the results are still out.

The next four chapters provide mini-biographies and analyses of the music and songs of key performers. He argues that "Woody Guthrie did it earliest and most convincingly, Pete Seeger did it the longest, Joan Baez did it most artfully, Phil Ochs tried the hardest, and young Bob Dylan did it best." The contradictions in Dylan's personality and career which eventually turned him into a musical celebrity are particularly well explained.

After 1968, muckraking lyrics fade as rock music in its varieties of acid-rock, folk-rock, and country-rock swallow folk protest. If the new dawn never

came, Rodnitzky feels it is fair to say the folk protest singers helped "to stave off that final night they warned of between the lines of their songs."

The book is well researched and sensibly presented, and the topic is one all modern American cultural historians should take seriously.

CHRIS SMITH
Arizona State University

BRUCE MIROFF. *Pragmatic Illusions: The Presidential Politics of John F. Kennedy*. New York: David McKay. 1976. Pp. xvii, 334. Cloth \$9.95, paper \$4.95.

The Kennedy administration has been the subject of many more books than its brief existence would suggest. A considerable number of them fall into two groups: friendly memoirs by participants and allies who are liberals; sharply critical, debunking studies by journalists and polemicists, who are generally much further to the left. Bruce Miroff, a political scientist who studied under Michael Rogin at Berkeley and who now teaches at the University of Texas at Austin, has obviously been persuaded by the latter group. He presents Kennedy as a cautious, uninspiring, conventional liberal—indeed, a conservative—at home, and as a cold warrior, imperialist, and adventurer abroad. In the introduction, he castigates recent studies of the presidency by Erwin Hargrove, James David Barber, George Reedy, and Arthur Schlesinger, jr., as "soft at the core . . . still in love with the Presidency." No such affection grips Miroff. His heart belongs to Gabriel Kolko, Howard Zinn, David Horowitz, Ralph Stavins, I. F. Stone, and other leftist critics of Kennedy, liberalism, and American institutions. In effect, Miroff concisely synthesizes polemical writings about Kennedy though he never actually says so. Newcomers to the field of recent history may find his book a useful introduction to the leftist critique, but experienced scholars will discover nothing new here.

This highly opinionated book frequently supports its arguments by citing other highly opinionated studies, occasionally by citing a memoir or public document, and seldom by citing a manuscript or oral history source. Because of government secrecy, many vital documents relating to recent foreign and military policies are unavailable, so we are bound to get fairly speculative studies. But Miroff presents his opinions as if they were definitive truths, glosses over conflicting opinions of value, such as Graham Allison's interesting work on the Cuban missile crisis, and ignores a whole body of relevant writing on Soviet foreign policy which tends to contradict his benign, some would say naive, view of the Kremlin. Well before this book was published, the Kennedy

Library had opened a large quantity of important primary source materials dealing with Kennedy's political career and domestic policies. Miroff barely scratches the surface of these sources. On civil rights, for example, he merely echoes the charges of Howard Zinn and Victor Navasky while failing to test their views against the abundant, available evidence. In this area, as in many others, detailed research into the primary sources at the Kennedy Library and elsewhere would have been essential to make this book work, but Miroff fails to conduct it.

CARL M. BRAUER
University of Virginia

DORIS KEARNS. *Lyndon Johnson and the American Dream*. New York: Harper and Row. 1976. Pp. xii, 432. \$12.50.

Psychobiographies have an irresistible appeal in this generation, for they satisfy a widespread wish to know "all," as well as simply to have "explanation." The needs of presidents in this democratic time and the relentless gaze of the communications media have opened the chief executives to fuller treatment than ever in the past, creating a new genre for attempting to understand the leadership of the nation. Doris Kearns has written her book in this mode.

Kearns had unique access to Lyndon Johnson, for as a presidential fellow in his White House beginning in 1967 she became a confidante and confessor of LBJ. As such, she listened to his recitations of intimate dreams as well as his private explanations of various national episodes. Her own wider-ranging knowledge of the Johnson literature allows her to draw into her running account of Johnson's success and agonies "insider" morsels that others have reported. (The footnotes are, however, not always specific as to page. They have some errors of attribution, including a reference to an address allegedly delivered by Johnson before the AFL-CIO that does not appear in the published Public Papers.)

Lyndon Johnson's reputation as a domestic reformer is secure, despite all his shortcomings in implementing them, which he was not loath to admit privately. But the dark shadow of Vietnam obscured his domestic labors. Moreover, Johnson was not willing to allow domestic achievement alone to be his passport to a high place in history: he would triumph abroad as well as at home. And as time passed he came to rely more and more on History for vindication. He would even try to force its hand, as he had forced the hand of Congress on a hundred occasions—and as he tried to force the hand of Ho Chi Minh. Johnson's penchant for talking to historians, therefore, was never solely in the interest of Clio herself, as this reviewer had

ample opportunity to observe at first hand. Kearns' intriguing work is an important book, but whether she has reported on Johnson's conscious self or on his unconscious is a question that will torment his future biographers.

HENRY F. GRAFF
Columbia University

CANADA

FRANÇOIS-MARC GAGNON. *La conversion par l'image: Un aspect de la mission des jésuites auprès des indiens du Canada au XVII^e siècle*. Montreal: Les Éditions Belarmin. 1975. Pp. 141.

The Jesuit missionaries in Canada in the seventeenth century relied on religious art as a supplement to preaching in their effort to convert the Indians to Christianity. This aspect of their mission had been little studied until the appearance of this work by Francois-Marc Gagnon, an art historian at the University of Montreal. This is a narrowly conceived study, based largely on the *Jesuit Relations*. Gagnon succeeds in showing the use of art by the Jesuits, but does not place their mission in Canada in the context of the Jesuit worldwide effort or of the intensive missionary work in Europe, which was so important an aspect of the Catholic Reformation.

The use of paintings and engravings by missionaries was fairly widespread and was stimulated by the Indians' fascination with the vividly realistic portrayals of religious subjects which they saw in the missionaries' quarters. The Jesuits, believing "fear is the forerunner of faith in these barbarous minds," (Père Le Jeune, cited p. 23) concentrated their message and imagery on eschatology. They emphasized the terrors awaiting the unbaptized after death, contrasted with the bliss of the faithful. As a result of this, no doubt, the Indians came to associate the Christian sacraments with death. Gagnon points out, in his interesting conclusion, that the Christian images were identified in the Indians' minds as evil fetishes, with power to kill. The inability of the sophisticated Jesuit priests, permeated with classicism, to grasp this aspect of the Indians' religious mentality meant that their mission could not succeed.

This is a valuable monograph which sheds light on a limited but interesting aspect of the Jesuit mission in Canada.

JONATHAN L. PEARL
Scarborough College,
University of Toronto

GREGORY S. KEALEY and PETER WARRIAN, editors. *Essays in Canadian Working Class History*. Toronto: McClelland and Stewart. 1976. Pp. 231. \$4.95.

To develop "a new distinctive synthesis of Canadian history" is an ambitious undertaking; yet that is the task which the young Marxist historians who compiled this collection set for themselves. Thus they have introduced to Canadian scholarship the forms of explanation which have transformed labor history in Britain and the United States. Focusing on the cultural habits of workers and the manner in which those habits conditioned the workers' response to industrial capitalism, the essays explore such themes as collective violence, strikes, fraternal organizations, and popular culture.

Like all such collections this one is uneven in quality. The findings of Michael Doucet's essay on working-class housing in Hamilton are essentially trivial. And Wayne Robert's attempt to explain the response of Toronto printers to change in terms of their artisanal heritage is obscured by a persistent ambiguity about the nature of the typographers' consciousness. The other essays in the collection, however, represent important contributions to Canadian labor history. Harvey Graff demonstrates the creative way in which quantitative techniques can be applied to the study of working people. Although some scholars would object to his measures of literacy, he argues persuasively that, despite liberal convictions and official orthodoxy, there was little correlation between social mobility and education in nineteenth-century Ontario.

The best essays are by Jean Morrison and Brian Palmer. Morrison has employed Herbert Gutman's model of culture and conflict to produce a sensitive and perceptive study of Lakehead freight handlers, though the timidity of her conclusion is rather disappointing. She demonstrates that British immigrants behaved differently in the face of CPR and CNR exploitation and provocation than did their southern-European workmates, but an anachronistic conception of workers prevents Morrison from drawing the conclusion which proceeds logically from her analysis—that the cultural heritage of Greeks and Italians was the critical variable in every case of violence. Palmer's essay sets out to explain the dramatic street-railway strike in London, Ontario by explaining "the social and cultural matrix" of the city's labor movement at the close of the nineteenth century. After presenting an excellent examination of working-class culture, he asserts that the artisans' traditions of mutuality, self-help, and freedom formed "the aspiration of a class," the workers' determination to control the job. This provocative construct, based upon the work of David Montgomery, may result in the recasting of conventional interpretations of job-conscious unionism, but it demands considerably more proof. Indeed, even though Palmer has done exhaustive and meticu-

lous research in local newspapers, in places his analysis seems inferential.

Despite its central Canadian bias, this collection is an auspicious beginning for the new labor history in Canada.

A. R. MCCORMACK
University of Winnipeg

ROBERT CRAIG BROWN. *Robert Laird Borden: A Biography*. Volume 1, 1854–1914. Toronto: Macmillan of Canada. 1975. Pp. xii, 306. \$14.95.

Despite Canadian historians' reputed propensity for biography, particularly political biography, there has existed until now no biographical study of Canada's eighth Prime Minister, Robert Laird Borden, the man who guided Canada through the traumatic Great War years. Robert Craig Brown's book, the first of a projected two volume study, covers the years from Borden's birth to the First World War.

Robert Borden was born at Grand Pré, King's County, Nova Scotia when the Fundy was still very much a northern extension of New England. After teaching (1873–74) at the Glenwood Institute, New Jersey, he returned home to study law. His legal training completed, Borden opened a law practice in the "provincial hinterland" before moving in 1882 to the provincial capital. In a law firm which boasted Charles Hibbert Tupper as a partner and which had only recently lost the former Premier, (and later Prime Minister) J. S. D. Thompson, to the bench, Borden was favorably situated to make the most of his talents. In less than fifteen years he possessed an income of \$30,000. In 1896 he successfully contested the federal constituency of Halifax. Less than five years later Borden was leader of the Conservative Party. A decade later he was Prime Minister, a post he held for almost nine years. The biographer's task, then, is to explain the peculiar course of Borden's career.

Although well written and based on a thorough reading of the relevant primary and secondary sources, this study is limited by the questions the author has asked. Unfortunately Brown's discussion of Borden's early years goes little beyond what is already known. His description of Nova Scotia suggests a lack of a sense of place. Moreover, without delving in the depths of psycho-history, one would like to know more about Borden's relationship with his father. Did Borden have an unofficial life? Did he ever meet Sir Wilfrid Laurier outside the Commons Chamber? Apart from brief and often intruded family scenes, Brown reveals little of Borden's personal life. Would not a closer examination of Borden's pre-1896 financial career have made the Tupper's choice of Borden as Conservative leader appear somewhat less idiosyn-

cratic? Alas, in this gentlemanly study money rarely changes hands. It is unfortunate that Brown did not provide as thorough and as convincing an analysis of the disintegration of the Quebec Conservative Party under Borden's leadership as he did of Borden's involvement in the *La Presse* affair. And whatever happened to Borden's vaunted "progressive" philosophy and his attempts to organize his party onto the treasury benches? Were both so easily forgotten under the exacting tutelage of the Provincial barons and the Toronto eighteen? Is there really nothing more to Borden? Those who have witnessed the explosion of that most famous of Canadian political clichés, the reputed dullness of W. L. M. King, may be excused if they are not entirely convinced.

CARMAN MILLER
McGill University

JOHN A. HOSTETLER, *Hutterite Society*. Baltimore: Johns Hopkins University Press. 1974. Pp. xvi, 403. \$14.00.

DAVID FLINT, *The Hutterites, A Study in Prejudice*. Toronto: Oxford University Press. 1975. Pp. viii, 193.

In table prayer be serious
And fold your hands in love,
Always in reverence lifting up
Your heart to God above.

.....

When yawning quickly raise your hand
To hide your mouth from view,
So that your neighbor need not fear
You will devour him, too.

John A. Hostetler and David Flint have described and analyzed why Hutterites in the United States and Canada have survived as a community when other such communities have dissolved. Jacob Hutter was the acknowledged leader of a Moravian Anabaptist group for only three years. Yet before his martyrdom in 1536, he impressed his ideas and his name on a small body of believers. Hutter taught his followers to resign themselves to tribulation in this life and to accept the necessity for communal living, as a means of working fearlessly in the face of external threats and internal problems of jealousy and division. After years of wandering and persecution, the first four hundred Hutterites moved to South Dakota in 1874. From there they branched into North Dakota, Minnesota, Montana, Washington, and Manitoba, Saskatchewan, and Alberta, until they now number about 22,000 people in 229 colonies.

The little verse with which this review begins may be found in the "Table Rules" in one of Hostetler's numerous and supportive appendixes. While the poetry may not be very good, it does

illustrate that these sturdy people are not always serious, and it gives a clue as to why and how they have survived. They have defined the community as an Ark, not as Utopia or as a communitarian experiment. On the basis of uncompromising beliefs and a comprehensive system of socialization believers move with folded hands, literally as well as figuratively, from cradle to the table to the stable, and finally to the grave. They have shown biological vitality, skill in the introduction and management of innovation, and a grave but genuine concern for delinquents. Hostetler has studied first-hand and written sympathetically about this radical Christian community and its conception of "total Christianity." His volume, now essential to any study of Hutterites, combines historical description, an analysis of social and cultural organization, and an examination of the problems and techniques of survival. While Hostetler, and Flint as well, might have given us a better psychological profile of these people, the former does include an exploration of mental health in the community. Hostetler concludes that the Hutterite rate of mental illness is much lower than in the population in general and that there is an effective psychotherapy at work. Appendixes include, among other things, an historiographical essay and selected bibliography.

Flint focuses on Hutterite survival amid hostility not only in Europe, but in the United States and in Canada. Writing of the beliefs and everyday life of Hutterites, particularly those of Pincher Creek, Alberta, the author covers much of the same material as does Hostetler. Flint studies how Hutterite refusal to participate in armed combat in World War I caused citizens in the United States to suspect them of disloyalty and how success in Canada in the expansion of their communal property stimulated Canadian jealousy and restrictive legislation. Hutterite pacifism and communalism are those convictions which gall people in whose midst the Hutterite Ark floats and often thrives economically.

Both of these volumes are attractively printed and illustrated with maps, charts, and scenes of Hutterite life, even of charming, chubby children at the dinner table. Flint has included twenty-eight pages of lesson plans to make his book useful for study and discussion.

JAMES H. SMYLLIE
Union Theological Seminary,
Virginia

M. JAMES PENTON, *Jehovah's Witnesses in Canada: Champions of Freedom of Speech and Worship*. Toronto: Macmillan of Canada. 1976. Pp. xi, 388. \$12.95.

Since their organization in Canada around 1890, Jehovah's Witnesses have undergone more consis-

tent opposition and persecution than any other religious denomination in Canada. This opinion of M. James Penton is one with which most historians must agree even while seeking the reasons for such widespread animosity. These reasons are not hard to find. Pacifists during the two world wars, the Witnesses had many fellow sufferers under Canada's conscription laws, but unlike such bodies as the Mennonites, the Witnesses were (and are) fighting pacifists under an apocalyptic imperative to challenge worldly authority with civil disobedience. In most societies that is a dangerous practice, but in Quebec during the quasi-fascistic rule of Maurice Duplessis from the 1930s to 1950s it verged on suicidal. Quebec's abusive use of its notorious Padlock Law drew to the Witnesses the grudging admiration, even sympathy, of many Canadians who were themselves disturbed by the apparent disloyalty of the sect.

When an author is an avowed member of the group he is examining the reader may reasonably expect some special pleading, but the opposite is true of this account of the Jehovah's Witnesses. Penton, a professor of history at the University of Lethbridge, handles his controversial themes with exceptional objectivity. His use of court records and contemporary newspaper accounts documents his case convincingly and effectively as he traces the Witnesses' history from persecution to toleration. Ironically, they have won that status by the efficient use of the very courts whose authority they otherwise deny. Penton's final chapter, "Abstain from Blood," deals with the major remaining area of the sect's conflict with the nation-state and public opinion. On the morality, legality, and utility of refusing blood transfusions, he marshals an array of scientific evidence which, if it does not convert readers, at least should cause them to reconsider the issue.

This well researched, attractively produced, and well-written history, whose author combines the understanding of a believer with the objectivity of a scholar, is an important contribution to the study of religion in North America and will remain a classic reference in this field.

JOHN S. MOIR
Scarborough College
University of Toronto

LATIN AMERICA

COLIN STEELE. *English Interpreters of the Iberian New World from Purchas to Stevens: A Bibliographical study, 1603-1726*. Oxford: Dolphin Book Company. 1975. Pp. 206. £ 7.00.

Colin Steele has reworked his doctoral dissertation into a small volume concerned with Spanish and

Portuguese works, translated into English, that throw light on various aspects of the New World. The subtitle of his book, "A Bibliographical Study, 1603-1726," delineates the period and purpose of the work. He has provided a useful manual of Iberian travel literature in this period that found its way into English. Although he mentions other types of writing, his emphasis is upon reports of travelers, geographers, officials, missionaries, and other folk who at first- or second-hand were concerned with Latin America.

Steele's labors overlap the work of others in the field, especially in the early chapters where he repeats much that is well known about the contributions of Hakluyt, Purchas, and their contemporaries. A chapter on "Oliver Cromwell and the Barren Years" requires forty-two of his 206 pages and indicates that the period was somewhat less barren of English interest in translations of Iberian works than the heading would suggest.

Steele's most original contribution is a chapter on "John Stevens and the Silver Age of Travel," which attempts to rehabilitate the reputation of Stevens as a compiler of travel narratives and as a translator of Spanish works on this subject during the first quarter of the eighteenth century.

A chapter on "Libraries, Catalogues and Collectors" is included to show the interest in Spanish and Portuguese travel and related literature in the period 1603-1726, as revealed by the works that English readers bought. An appendix provides "A Bibliography of English Translations of Spanish and Portuguese Books on the Iberian New World, 1603-1726."

For purposes of a doctoral dissertation, the chronological and subject restrictions are understandable, but for general use by scholars, Steele's book is limited. Of necessity he has had to leave out much valuable peripheral material that does not fit into his definitions. Nevertheless, one should not quarrel with an author about what he did not intend to do, and we can be grateful for a convenient listing of many obscure works.

LOUIS B. WRIGHT
The National Geographic Society

URSULA EWALD. *Estudios sobre la hacienda colonial en México: Las propiedades rurales del Colegio Espíritu Santo en Puebla*. Translated by LUIS R. CERNA. (El proyecto México de la Fundación Alemana para la Investigación Científica, number 9.) Wiesbaden: Franz Steiner Verlag. 1976. Pp. xix, 190. DM 98.

This is a carefully researched study of the extensive rural properties owned by the Jesuit college in Puebla (Mexico) during the Spanish colonial period. It is published as volume 9 of a solid series of West German studies of the Puebla-Tlaxcala region sponsored by *Das Mexiko-Projekt der Deutschen*

Forschungsgemeinschaft. In addition to its place in this regional, cross-disciplinary project, the book adds to the growing literature on the economic activities of colonial estates, in particular, to the recent studies of Jesuit and ex-Jesuit haciendas by Hermes Tovar Pinzón, Herman Konrad, James Riley, and Bradley Benedict.

The five core chapters (chapters 5-9) deal with the college's three clusters of rural properties: near the city of Puebla, northeast of Puebla in the Huamantla area, and in the Mixteca Baja. For each estate, the author describes the location, chronology of land acquisition (revealing surprisingly early formation and little expansion after 1650), resources and improvements, economic activities, labor supply, and the fate of the estate after the expulsion of the Jesuits from Latin America in 1767. Data on production, manpower, expenses, and revenues for some of the estates are included in forty-eight tables. The introductory and concluding chapters touch briefly on more general topics such as the place of rural estates in the colonial economy.

The main thesis is that the Jesuits, starting with very little capital in the late sixteenth century, developed their wealth in rural estates by taking underused properties in strategic locations and exploiting them systematically. They wisely divested themselves of useless lands donated to the college, rented lands near their profitable estates as the market warranted, practiced specialization (wheat on one estate, pigs on another, goats on a third, etc.), and transported their own produce. This is not a new insight—François Chevalier developed the same point in a general way twenty-five years ago—but it is elaborated here in revealing detail and refined in one important aspect. Where Chevalier and others speak of the Jesuits as "agronomists" and "great farmers," Ewald demonstrates quite convincingly that the Jesuits should, instead, be considered able "*empresarios Agrícolas*" (rural businessmen). Their genius in turning a profit derived from management skills, not the practice of "scientific" agriculture. They followed traditional methods of farming and ranching, showing little inclination to experiment with new crops or to try new ways of improving yields, even in the years preceding the expulsion.

The discussion of Jesuit management is excellent, and the wealth of new data presented can serve as a reference point for others working in the economic history of rural Mexico, but this is a book with many loose ends. The tables serve up isolated chunks of information on the activities of individual estates at different periods. Rarely are these tables interpreted in the text or compared to tabulated information in other chapters. A synopsis chapter is needed to combine comparable eco-

nomic data from the various estates into a coherent evaluation of productive activity over time. Too often, the reader is left with vague, sometimes misleading statements about matters which are well enough documented for individual properties. This is true, for example, of resident versus day-wage labor. The author suggests that resident labor was the Jesuits' standard means of insuring an adequate labor force (p. 29), yet there are pockets of information in chapters 5-9 which, taken together, seem to indicate that nonresident day and seasonal farmhands from nearby villages met most of the estates' needs.

WILLIAM B. TAYLOR
University of Colorado,
Boulder

ROBERT J. KNOWLTON. *Church Property and the Mexican Reform, 1856-1910*. (The Origins of Modern Mexico.) DeKalb: Northern Illinois University Press. 1976. Pp. xii, 265.

In the mid-nineteenth century, Mexican Liberals launched a frontal assault on corporate privilege and land ownership. Their aim was to foster individualism and create small- and middle-size farms by first disamortizing Church property and then, when their efforts met violent resistance, by nationalizing it. This Church-State conflict, which continued with varying intensity until late in the century, is a central theme of Mexican history. The present work focuses on the legal aspects of the attack on Church wealth and complements existing economic studies.

After indicating the sources of the conflict in the first chapter, Robert J. Knowlton systematically traces its convoluted and complicated legal history from 1856 to 1910, always placing it in the context of more general events in the country. He draws upon his extensive archival research in Mexico and the United States to make the best current estimate of Church wealth and governmental proceeds derived from it. Laws were inadequately drafted; records were poorly kept; wars intervened; and governments regularly toppled.

The Liberals failed in the execution of the laws and in placing too much faith in individualism. Wealthy individuals created lay latifundia and the Church gained strength during the Porfiriato. The Revolution would have to attack the problem anew. This generally accepted thesis is amply proven.

Nevertheless, Knowlton overstates the inadequacies of the laws by not putting equal emphasis on the sheer capacity of the conservatives to resist, and misleads the reader by not explaining that the Porfirian Church was substantially weaker than its pre-Reform predecessor and no

longer dominated the land. The failure of the Liberals was not as severe as he suggests.

This excellent book provides a broad framework within which to place future studies and to demonstrate the complexities and difficulties of social reform.

DONALD J. MABRY
Mississippi State University

RODNEY D. ANDERSON. *Outcasts in Their Own Land: Mexican Industrial Workers, 1906-1911*. (The Origins of Modern Mexico.) DeKalb: Northern Illinois University Press. 1976. Pp. xxiii, 407. \$15.00.

In the 1880s and 1890s, galvanized by foreign investments, Mexico turned predominantly capitalist. By the 1900s both nationalism and "the social question" had erupted. Created in this development, industrial workers proved particularly troublesome. Rodney D. Anderson's book is an account of their emergence and public action.

Drawing largely on the Colegio de México's research, Anderson argues that "industrial progress" brutalized workers' living and working conditions and wounded their "pride and self-esteem." Workers therefore organized not as a dispossessed class but as a disparaged class.

From his own extensive research in primary sources, Anderson presents the crux of the story in three great strikes in 1906. The struggles themselves remained within established bounds, protests not against capitalism but against foreign and native industrialists' "mistreating" Mexican workers. But the end of one strike was terrible. At Río Blanco in January 1907 textile workers rioted, and the army crushed them, killing "fifty to seventy." The impact on working-class organizations, the government, business, the Church, and budding revolutionaries was traumatic. Because "working Mexicans" obviously could not get due protection from the government, its legitimacy faded. Simply by demanding equality with other classes, workers helped to transform their society.

Thus "the labor question" ranks with "the agrarian problem" and resistance to "dictatorship" as a major cause of the regime's downfall. In the presidential campaigns of 1909-10 the opposition used it against the government. And the revolutionaries of 1910-11 did too. In both cases workers mattered, as symbols of nationality disgraced and as a potential base for revolt. The revolution owed its victory "in an important way to the Mexican worker."

Several weaknesses diminish the book's force. For example, Anderson has researched no company records, neglects the old mining centers, ignores the oil fields and the ports. He disregards gross chronological and sociological differences

among workers. And he fails to analyze the subject that most concerns him, working-class mentality. If workers declared they were only patriotic citizens, he takes them at their word.

But overall the book is strong and will endure. Though it does not entirely replace previous studies, it is fuller, sounder, and clearer. Not narrow "labor history," it gives a broadly focused and often richly textured portrait of the first phase of Mexican proletarian history. And better than any book before, it conveys the vision prerevolutionary workers claimed they had of themselves.

JOHN WOMACK, JR.
Harvard University

MANUEL MORENO FRAGINALS. *The Sugarmill: The Socioeconomic Complex of Sugar in Cuba*. New York: Monthly Review Press. 1976. \$23.50.

This valuable study of the Cuban sugar industry from 1760 to 1860 is the first of three projected volumes which will eventually reach to the 1960s. In his wide-ranging work, Manuel Moreno Friginals is concerned not only with sugar itself, but with the social, political, technical, ecological, intellectual, religious, and other closely-related aspects of Cuban history.

The author, trained in history and having some ten years' technical experience in corporate management, has brought his skills to bear, in the form of a Marxist analysis, on a large body of data previously untouched by historians. Included are sugar mill ledgers and other original documents located in Cuba's Biblioteca Nacional and Archivo Nacional and also oral information from sugar industry personnel with experience going back to the 1870s. This new material complements what we have learned from Franklin W. Knight's *Slave Society in Cuba during the Nineteenth Century*, as the two authors approach many related issues from different perspectives based on different data.

Moreno Friginals traces the sugar industry from the small and inefficient *trapiches* of the eighteenth century to the fully mechanized mills of the 1860s. He carefully computes the production of each type of improved mill and provides rich detail concerning each technical advance—not only major ones such as the steam engine and the railroad, but many lesser ones such as the vacuum pan, a costly invention which more than others meant that "the small sugar mills could no longer compete with the big ones and were doomed to disappear" (p. 113).

The author devotes considerable attention to the development of a Cuban labor pool, first slave and then free, and documents the generally wretched labor conditions on the plantation and in the mill.

One of the author's main theses is that slave labor was incompatible with most technological advances and that the need for these advances in the highly competitive sugar industry led first to enclaves of free laborers in a slave-dominated production process and eventually to the abolition of slavery. In a related aspect of his work, the author details the class and personal interests of the Cuban intellectual spokesmen for agricultural expansion and improvement, for sugar, for slavery, and later for the changeover to free wage-labor (the brilliant Francisco Arango y Parreño is the classic example).

A wide range of scholars, including those interested in the Caribbean, Cuba, sugar, plantation agriculture, and slavery, should welcome Moreno Friginals' *The Sugarmill*. Not only is the original work a unique and serious contribution in a field yet little explored, but this excellent first translation of *El Ingenio* is backed up with profuse and useful illustrations and an index. It is with pleasure that this reviewer awaits the publication of the promised subsequent volumes.

BRUCE J. CALDER
University of Illinois,
Chicago Circle

SAMUEL FARBER. *Revolution and Reaction in Cuba, 1933-1960: A Political Sociology from Machado to Castro*. Middletown, Conn.: Wesleyan University Press. 1976. Pp. xix, 283, \$15.95.

This volume by Samuel Farber, a political sociologist, is a reasonable attempt to explain the background of the Cuban Revolution. Although the author lived in Cuba until the age of eighteen, he indulges in relatively few polemics and concentrates on explaining what happened in Cuba during the crucial years from 1933 to 1960.

Farber argues that various "class struggle" interpretations of the revolution are incorrect, and that the Cuban experience cannot be viewed as a worker, peasant, or middle-class phenomenon. Instead, he turns to Karl Marx's theory of Bonapartism as a framework for interpreting Cuban history after 1933. According to this concept, "...classes or groups with a low degree of cohesion, of economic and political organization, and of political consciousness are susceptible to authoritarian leaders" (p. 18).

Farber traces the political and social history of Cuba after 1933 to develop the theme that no one social class or political party could achieve hegemony. Thus, Fulgencio Batista played the role of a "conservative Bonapartist" except for the brief (and artificial) interlude of "constitutional politics" between 1944 and 1952. Both Batista's easy seizure of power in 1952, and Castro's victory (and

rapid establishment of a left-wing totalitarian regime) were based upon the same basic factors of weak social classes and political organizations. Both men were *declassé* power brokers operating independently of traditional groups.

This synthesis of work by historians and social scientists is an intensive presentation of the well-known "invertebrate society" thesis. Farber rightly criticizes those social scientists who ignore history, but his own work does not escape from this defect. For example, he does not come to grips with the historical reasons for the weaknesses cited. In typical Cuban fashion (past and present) he blames U.S. imperialism for all Cuban problems, and quotes a paragraph written by this reviewer (describing the Reciprocal Trade Agreement of 1934) to "prove" that a "pattern of economic stagnation" was deliberately forced on Cuba by the North American "ruling class." This economic situation in turn caused all of the social and political problems. Economic factors were important (although more complex than Farber realizes), but one cannot understand Cuban social and political weaknesses after 1933 without knowing something about the hundred years of developments prior to that time; 1933 might have been a "watershed," but it was not the entire pond.

In summary, this book has all of the merits and defects of single-factor analyses, but it is a useful work and deserves serious consideration by historians.

ROBERT FREEMAN SMITH
University of Toledo

JOHN V. LOMBARDI. *People and Places in Colonial Venezuela*. Bloomington: Indiana University Press. 1976. Pp. xiv, 484. \$25.00.

The Bourbon reform of the Spanish empire included a far-reaching reorganization of local reporting that has left huge masses of statistical information, still largely untapped by historians. As elsewhere in Europe, the Spanish state tried to elicit from the Church and Church registers reports, which remain in episcopal archives. John V. Lombardi's book is a notable presentation of extensive research in the archive of the archbishopric of Caracas. He has located, reduced to common statement, and analyzed by computer a mass of parish reports, mostly annual, for the years 1771 through 1838. They provide incomplete but substantial coverage of population in the bishopric of Caracas, a fifth of present-day Venezuela in area, but which included three-fifths of the total population at the time. It is a noteworthy achievement.

This volume, the first in a series, is basically in two parts. The second and longest one tabulates,

parish by parish, the data by ten-year averages for the basic categories of population by sex, race, proportion of children, married persons, and clergy. The first part of the book concentrates upon the thirty-five percent of the parish returns that fall in the years 1800–09 and give a baseline just before the convulsions of the Wars of Independence. Two appendixes tabulate the basic data, extracted from the second part, and provide a master parish and locality name file, with maps.

Six chapters, provided with ample well-designed graphs, discuss in unusually thoughtful, lucid, and urbane prose the nature of the reporting, the meaning of terms, the resolution of problems, the techniques of analysis, and the meaning of the data when analyzed in terms of regional and rural/urban groupings, age, racial, and marital categories, etc. Students of slavery will be impressed by the marital statistics for slaves.

Chapter 6 concentrates upon one city of nearly ten thousand, San Carlos de Austria, as an example of what the Wars of Independence meant in demographic terms. The city lay in the *llanos* at the crossroads of a series of routes linking royalist Coro and Maracaibo with the revolutionary central highlands and valleys. Not surprisingly, white population fell sharply; black population fell distinctly less; single people of all races tended to leave.

In his conclusion Lombardi points out that by 1800 Caracas already enjoyed a predominant position and that the Venezuelan network of towns and cities, which would endure until the petroleum boom of the 1930s, was already in place. This is a most useful and notable book, one that automatically is an indispensable source of data for other kinds of studies.

WOODROW BORAH
University of California,
Berkeley

HENRY F. DOBYNS and PAUL L. DOUGHTY. *Peru: A Cultural History*. (Latin American Histories.) New York: Oxford University Press. 1976. Pp. 336. \$12.50.

About midway in their short book (266 pages of text) Henry F. Dobyns and Paul L. Doughty have gotten only to the beginning of the Peruvian independence movement. It is good that they lingered in the early period, for the two anthropologists deal expertly with the pre-Incaic and Incaic periods, the introduction of the Spanish "Conquest Culture" from 1532 to 1572, the era of "Classical Colonialism" 1569–1700, and the turbulent eighteenth-century years that witnessed attempts both at colonial rebellion and imperial reform.

Perhaps the strongest feature of the book is the

treatment of the interaction in the colonial period between Indian and Spanish cultures. For a succinct summary of this complex but vitally important phase of Peru's history that has continued to influence its development to the present time, one could do no better than begin with this book. In treating this subject, as in dealing with other facets of Peruvian history, the authors display even-handed impartiality.

It is worth remarking that the two most recent attempts at brief histories of Peru have both been at their best in dealing with the colonial period. The other work I have in mind is Luis Martín, *The Kingdom of the Sun: A Short History of Peru* (1974). The works complement each other nicely for Martín is strongest in dealing with the Spanish cultural establishment, while Dobyns and Doughty stress the interplay of two cultures—neither of which was in the remotest way monolithic.

The underlying causes of nineteenth-century political turbulence (assuming such causes exist) are largely ignored by Dobyns and Doughty, but they do make occasional references to the difficulties of bridging the cultural gulf separating Indian Peru and "white" Peru, the latter coming increasingly to be populated by mestizos. Ending their treatment of this century, they note that Peru was preparing to enter the twentieth century, one that was dominated by European and North American mechanical technology, while looking back toward its agrarian past.

By the time their history takes up the story of the twentieth century, the authors have only some sixty pages remaining to them, so many topics are neglected, among them the vast changes produced during the Augusto B. Lequía *oncenio* (1919–30). In describing Peru since the 1968 military takeover, they virtually abandon the search for underlying currents and concentrate too much on various government agencies, some of which have already passed out of existence or lost all vitality. Quite properly, though, they stress the importance of agrarian reform since 1968; and they draw attention again to a previously emphasized theme, the myth of Peruvian underpopulation. They end by noting that Peru is an ancient society and that longstanding environmental, biological, and cultural patterns limited the extent of change achieved by the conquering Incas and Spaniards. Further, they question whether the new military rulers can break the historic mold and define a different future.

In all, this is a good if somewhat uneven book, with a welcome stress on demographic factors and enhanced by a political chronology chart, statistical tables, and an excellent bibliographical guide. For all its strong points, I wonder if the book will be as useful for undergraduate classes as

the ones on Argentina, Brazil, and Mexico published earlier in this series.

FREDRICK B. PIKE
University of Notre Dame

FREDERICK M. NUNN. *The Military in Chilean History: Essays on Civil-Military Relations, 1810-1973*. Albuquerque: University of New Mexico Press. 1976. Pp. xiv, 343. \$15.00.

In light of recent political events in Chile and the increasing number of military governments in Latin America, Frederick Nunn's current work is both interesting and important. In a well-researched and readable series of essays, the book traces the historical relationship between the military and Chilean civilian administrations and dispels the naive notion of an apolitical Chilean military. The book is divided into three sections: the first covers the nineteenth century to the revolution of 1891; the second centers on the professionalization of the military and its confrontations with civilian governments up to the Great Depression; the third deals with the modern period of confrontation between the military and civilian sectors leading to the present military government. Nunn concentrates on the creation, organization, and development of the political character of the Chilean military and its internal relations with civilian governments.

The book concludes that the Chilean military, because of its harmonious relationship with civilian governments, acts only in times of severe stress to take charge of administering the government. It is, however, hard to accept Nunn's view that the highly politicized military lacks a political ideology (pp. 296-97). The attitudes of the military from the Plan Linea Recta (1955) to the present junta are virtually the same and indicate the existence of an ideology. A social examination of the Chilean officer corps (not apropos in this political study) might shed some light on the ideology of the military. Of more interest would be a more complete analysis of the impact of the Cold War on military attitudes in Chile. The limited amount of discussion on this subject implies a lack of importance. It is difficult, however, to deny the success or importance of the Military Assistance Programs, counterinsurgency training, and anti-Communist education programs. A detailed analysis of the impact of the Cold War may help to explain the vicious and tragic attack of the military on the Unidad Popular government.

Those interested in understanding Chilean politics, especially civilian-military activity, will find in this series of essays a valuable first step in a subject of increasing importance to Latin American scholars. With this book Nunn establishes

himself as a leading expert on the Latin American military's political activity.

ROBERT OPPENHEIMER
*University of California,
Los Angeles*

HENRY H. KEITH and ROBERT A. HAYES, editors. *Perspectives on Armed Politics in Brazil*. Tempe, Arizona: Center for Latin American Studies. 1976. Pp. xxix, 258.

In 1889 the Brazilian military gave the aged Emperor Pedro II a one-way passage to Europe and declared Brazil a republic. It was an institutional rebellion as well as one of personalities. The military, mainly the army, was motivated partly by fear that the monarch was going to abolish the army. On an institutional level the army was acting to protect itself; on the individual level officers were protecting their source of livelihood, status, and promotion. They also felt that the army, having made the republic, had the duty to protect it. Brazilian grandeur, the functioning of the national government, and the status and role of the army became mixed in the thinking of successive generations of officers.

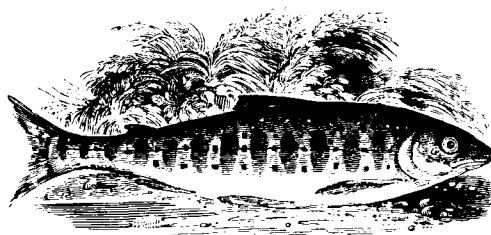
At the turn of the century the army acted as a police instrument for the civilian politicians while trying to dress itself in the professional garb of a European-style army. As the century progressed, officers, who had resented police activities as below their professional dignity, came to believe that only reform of the political system would allow Brazil to develop in such fashion as to permit the army to become the truly European-style force they believed was necessary to protect Brazil. Professionalism, national security and development, and political reform thus became basic to military thinking and motivation. The result has been a military-controlled government since 1964.

Perspectives on Armed Politics in Brazil proposes to trace the evolution of the military's role in political life. The seven contributors treat the formation and growth of the army from early colonial days to the mid-nineteenth century, the political activities of nineteenth-century officers, the army as an instrument of political intervention in the 1910s and 1920s, the political role of the São Paulo state police, the peculiar use of political violence in Rio Grande do Sul, the military club, foreign expeditions, the military's place in governmental policy-making, and the post-1964 governments. There is no doubting the importance of the themes considered, but the superficial nature of most of the essays and the odd failure of the authors to use important studies that have appeared in the last ten years detract from the book's usefulness. It is difficult to understand how one can discuss the activities of the Marquis of Lavradio in the eigh-

teenth century while disregarding Dauril Alden's study based on the marquises' papers and reports, the *Inconfidencia Mineira* without regard to Kenneth Maxwell's and José Ferreira Carrato's writings, the Prestes Column or Padre Cicero without reference to Neill Macaulay's and Ralph Della Cava's work or the military during the late empire without a nod to John Schulz's and William Dudley's dissertations and articles. With such omissions in the bibliographic research the essays are hardly contributions to historiographic development.

Collections of essays such as this have validity if they are soundly based, offer new interpretations or syntheses, and if each essay can stand as an independent article. These presentations do not meet these criteria. And even the most interesting—those of Henry Keith, Robert Hayes, and Carlos Cortés—suffer from the poor editing that scars the volume. Although there are useful photographs, charts, and a glossary of terms, the lack of an index further weakens the book's utility.

FRANK D. MCCANN
Universidade de Brasília.



Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. All communications should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor." Comments not intended for publication should be included in the covering letter. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

Robert H. Zieger, in his acidulous analysis of the Robinson and Bornet volume on Hoover's presidency in last October's review article (*AHR*, 81 [1976]: 800-10), not only has little good to say about the book but he has bad things to say about those who can see some merit in it. In the process he changes Robinson's middle initial to an "A," reproduces one quotation imprecisely, and quotes other statements out of context in such a way as to put the two authors in the most unfavorable light. He especially deplores the four "worthies," including myself, who were invited to provide comments for the dust jacket. He launches this attack in such a way as to make me seem responsible for the words of the other three "luminaries." My complete statement reads: "Exhaustively researched, impressively documented, and sympathetically written, this book will help swing the pendulum toward a fairer and more balanced appraisal of the thirty-first President of the United States."

The Robinson-Bornet offering is "exhaustively

researched," as Zieger paritally concedes, with careful attention given to the manuscript archives in Iowa and at the Hoover Institution. The book is "impressively documented," with sixty-two small-print pages of bibliography and footnotes, or about one-sixth of the total. And the volume is "sympathetically written," but Zieger evidently finds it too sympathetic. He concedes that the book may help swing the pendulum back, "but only for those who have read nothing on Hoover for the past ten or twelve years." This takes in tens of millions of adult Americans who prefer television.

THOMAS A. BAILEY
Stanford University

PROFESSOR ZIEGER REPLIES:

I plead guilty to misrendering the middle initial in Professor Robinson's name. While I am at it, I would also note that I misrendered the given name of Benjamin Weissman in the text, after citing it properly in the listing of books under review. (Still on the same subject, but parenthetical to Professor Bailey's complaint, may I note that the *Review* itself misspelled my name in two different ways, once in the squib on contributors in the October number and again in the annual index in the December issue.) This misrendering of names is serious and should be punished.

As for the substance of Professor Bailey's remarks, I believe that my review of the Robinson-Bornet book is sound. In no way have I been unfair to the authors or anyone else. Readers, especially those with access to a still dust-jacketed copy, can judge for themselves.

ROBERT H. ZIEGER
Kansas State University

THE EDITOR REPLIES:

The names now on the masthead page of the *Review* ought to be some guarantee that at least Germanic names will be spelled correctly in the future. As for past offenses, we are firmly opposed in principle to capital punishment.

OTTO PFLANZE

TO THE EDITOR:

Michael Rogin's review of my *American Indian Policy in the Jacksonian Era* in the October issue of the *AHR* (81 [1976]: 969-70) contains several statements which I challenge. Contrary to Rogin, my study does not "begin in defense" of Andrew Jackson's Indian policy. My book analyzes the origins, motivation, execution, and results of the Indian removal policy; it does not defend Indian removal. Although I quote Old Hickory's rhetorical defense of his Indian policy, I demonstrate that there was a wide gap between his rhetoric and the realities of that policy. Contrary to Rogin, I am not guilty of "ignoring the illegality of extensions of state law over the southern tribes. . . ." A perusal of my book will provide readers with ample references to this controversy and to the responses of federal officials, the Indians, and others as well. Indeed, my study sheds some new light on the case of *Worcester v. Georgia*. Finally, I must contest Rogin's claim that my examination of Indian policy is "bound by the world view of policy makers" and is "exculpatory and ahistorical" because it is not psychohistory. Whatever the merits of psychohistory, examinations of the formulation, execution, and results of Indian policy from the perspectives of both ethnohistory and public administration help to broaden our understanding of Indian-white relations.

In closing, let me note that I was greatly perplexed to see on page 44(a) of the advertisement section of the same issue that the publisher of the paperback edition of Rogin's psychobiography of Andrew Jackson has a quotation from a reviewer allegedly claiming that Rogin's book is "the first comprehensive evaluation of the administration of Indian affairs in the 1830s and 1840s. . . . Should be a standard account for some time." This statement by Leo E. Oliva in the *Library Journal* (May 15, 1975), p. 984, refers to my book not Rogin's.

RONALD N. SATZ
University of Tennessee,
Martin

PROFESSOR ROGIN REPLIES:

As if it were not enough for me to raise questions about Ronald Satz's volume on Jacksonian Indian policy, Vintage Books has apparently expropriated praise of his book to advertise mine. I had not seen the ad, deeply regret the indignity, and have asked Vintage for an accounting and an apology.

Professor Satz and I have written very different books; one would not have thought it possible to

confuse them. In his own *AHR* review of *Fathers and Children* (81 [1976]: 658-59), Satz claims that "Rogin's statements about Jackson's hatred of the Indians, and his defense of the Supreme Court decision in *Worcester v. Georgia*, among others, are refuted in my *American Indian Policy in the Jacksonian Era*." Without accepting either Satz's characterization of my views or his refutation of them, one nonetheless gets a better sense of the differences between us from that single sentence than from the reinterpretation of his own book Satz now offers. He does in fact portray Jackson's initial intentions sympathetically and does not acknowledge that the extension of state sovereignty over tribal lands clearly violated federal law. One need not advocate psychohistory (which the review does not mention) to see that Satz and the other authors reviewed exaggerated the gap between the good intentions and bad consequences of Indian policy because they remained too imprisoned in the rationalizations of policy-makers.

MICHAEL ROGIN
University of California,
Berkeley

VINTAGE BOOKS REPLIES:

Vintage Books would like to apologize to Ronald N. Satz and the University of Nebraska Press for an error in the Vintage advertisement appearing in the *Review* for October, 1976, according to which the quotation from the *Library Journal*—"the first comprehensive evaluation of the administration of Indian affairs in the 1830s and 1840s. . . . Should be a standard account for some time"—was mistakenly applied to Michael Rogin's book, *Fathers and Children*. The quotation described Satz's book *American Indian Policy in the Jacksonian Era*, which was reviewed jointly with the Rogin volume.

We are appalled by our mistake and have, of course, deleted the quotation from all later advertisements.

ANNE FREEDGOOD
Executive Editor

ERRATUM:

Through a printer's error the review of Philip V. Cannistraro *et al.*, eds. *Poland and the Coming of the Second World War* in our April issue was incorrectly attributed to Edward D. Wynot, Jr. The author of the review was Piotr S. Wandycz. Our apologies to Professor Wandycz.

THE EDITOR

Recent Deaths

RHÿS W. HAYS, professor of history at the University of Wisconsin, Stevens Point, died on February 13, 1976 at the age of forty-nine. Born May 16, 1926 in Cleveland, he grew up in New York City, and received a B.A. from Columbia in 1945, a B.D. from Union Theological Seminary in 1949, and his M.A. and Ph.D. from Columbia. Prior to joining the faculty at UW-SP in 1961, Hays taught part-time at Queens College and Bronx Community College, and studied at Cambridge and London Universities.

A specialist in medieval history, Hays was the author of *History of the Abbey of Aberconway, 1186-1537* (1963), and "The Welsh Monasteries and the Edwardian Conquest," in *Studies in Medieval Cistercian History* (1971), as well as other scholarly articles and contributions to the *New Catholic Encyclopedia*. An avid chess player and detective story fan, Hays also contributed to journals in those areas.

Although suffering from physical disabilities caused by polio as a child, Hays remained active professionally until his death. His many contributions to his chosen profession stand as a tribute to his devotion to historical scholarship. A scholarship for students in history has been established in his name at the University of Wisconsin, Stevens Point. And a volume of selected works by Rhÿs W. Hays, chosen from his writings in history and in the field of detective stories, will be published soon in his memory.

JUSTUS F. PAUL
*University of Wisconsin,
Stevens Point*

JOSEPH J. MATHEWS died on August 16, 1976 as a result of a fall that took place in his summer home at Cabot, Vermont. He had been a member of the Emory University faculty for thirty years and was about to begin a long anticipated retirement. Born in Campton, Kentucky on September 14, 1908, he graduated from Duke University in 1930. After taking an M.A. at that same institution, he taught

for a year at the University of Chattanooga before matriculating at the University of Pennsylvania where he received his doctorate in 1935. He taught for one year at Duke and then accepted a position at the University of Mississippi where he advanced rapidly to the position of acting dean of the graduate school. In 1942 he was given a commission in the navy and was put in charge of the wartime history project of the Bureau of Ordnance. Upon leaving the service in 1946 he joined the Emory faculty where he was named to a Charles Howard Candler chair in 1960. In 1966 he served as George A. Miller visiting professor of journalism at the University of Illinois, Urbana-Champaign.

Joe Mathews' first book, *Egypt and the Formation of the Anglo-French Entente of 1904*, appeared in 1939, but by this time he had begun to shift his interest from traditional diplomatic history to the role of the news media in international affairs. That same year he published articles with the suggestive titles of "Death of Press Reform in France," (*Public Opinion Quarterly*) and "The Radio and International Relations," (*Southwestern Social Science Quarterly*). Numerous other articles followed and in 1957 his longest book, *Reporting the Wars*, appeared. This pioneering achievement was more than a study of European and American war correspondents and of official news releases during the past few centuries, as the title suggests; it was a penetrating and thoughtful analysis of what freedom of information really is. There were times, Mathews discovered, when in the name of national defense the democracies were more insistent on wartime censorship than the authoritarian states, and in our century he found that the war correspondents' reliance on official releases posed a new threat to accurate war news. In his final book, *George W. Smalley, Forty Years a Foreign Correspondent* (1973), Mathews explored the role of the newsman in international relations and in molding public opinion. These and other works won him widespread recognition. He was a Guggenheim Memorial Fellow, a Fulbright Research Fellow to the United Kingdom, and a Fulbright Lecturer to Turkey.

Mathews also did yeoman's duty in administrative service to his profession. He was a member of committees of the American Historical Association, the Social Science Research Council, and the Southern Historical Association. In 1964 he became the only European historian who so far has served as the president of the last named organization.

Noteworthy as these achievements were, Joe Mathews will be remembered best by his colleagues as a kindly, farsighted academic statesman. He assumed the chairmanship of the history department in 1948, and during the decade that followed he was the principal architect and the guiding spirit in the development of its doctoral program. The creation of such programs was a familiar occurrence in the postwar era, but few began with so little (there had been only three members of the department prior to World War II) or were inaugurated with such limited funds. He was able to make only one senior appointment. His success was not only a tribute to his leadership and vision, but also to the countless hours he spent giving guidance and counsel to the younger members of the faculty around whom the department was built. He was an excellent teacher and won the hearts of students through his unselfish devotion to their interests. In 1970 as the time for his retirement approached and the academic marketplace blackened, he volunteered for the post of director of graduate placement. No act was more typical of his career. Upon his retirement, he was honored by his former students and friends with the creation of an endowed fund bearing his name, the income from which is used to aid Emory graduate students in traveling to archives needed for their dissertation research. He will long be remembered with affection by his numerous friends, his colleagues, and his students. He is survived by his wife, Marcia Mayfield, and his son, Timothy.

RUSSELL MAJOR
Emory University

LAWRENCE D. STEEFEL died in Minneapolis on October 28, 1976. For many of us he was a good and close friend, a wise and judicious counselor, and a trusted colleague. My personal loss is very great; he was my professor, my friend, and my colleague for the last forty-five years. My feelings are undoubtedly shared by a host of the friends of this gentle, humane, knowledgeable man. His colleagues at the University of Minnesota and in the historical profession at large, at the Campus Club and the Unitarian Church and in the wider communities of Minneapolis and St. Paul are all saddened by his death.

Lawrence D. Steefel was born in Rochester, New York, June 25, 1894 into a family in which

education and cultured living had high priority. He attended Phillips Exeter Academy and then Harvard, where he took three degrees: A.B., 1916; M.A., 1917; and Ph.D., 1923. The gap between the M.A. and the Ph.D. can be accounted for by the war of 1917-1918, in which he played a minor role. The army first assigned the young man who had just completed an M.A. in history to the care of horses! Then his Harvard professors remembered their bright student and asked that he be sent to assist them in the preparation of policy papers for President Wilson. The army complied, but neither the President nor his staff ever looked at the papers. The young scholar returned to Harvard to work with Robert H. Lord and A. C. Coolidge. He held in succession an Austin teaching fellowship, a Parker fellowship, and a John Harvard fellowship to finish his Ph.D. In 1921-22 he studied in European archives and libraries, and pillaged the shelves of German and Austrian second-hand booksellers. His Ph.D. thesis easily won the Toppan prize in 1924. From 1923 to 1959, when he retired as professor emeritus, Lawrence Steefel was a key member of the University of Minnesota's Department of History; those were the years when it came of age on the national scene. In 1925 he married Genevieve Fallon—attractive, intelligent, and enterprising—whose subsequent career in public service was impressive.

Unlike many scholars who resist all suggestions that they might teach anything outside of their "specialty," Steefel introduced courses in Russian history, the Ottoman Empire, the Religious Wars in France, the French Revolution, and German history, as well as in nineteenth-century European history. Graduate students will long remember his "Bib. and Crit." and Historiography which, despite the rigorous requirements, were popular and well attended. Eugene McCarthy is only one of the members of these two courses who has testified to their value; my own students had nothing but praise for them. His seminars, often held in his book-lined study, were vital and important experiences; he was one of the most widely read men I have ever known and he seemed to remember everything he had ever read. Those of us who were his Ph.D. students will remember that he did not use us as "research assistants"; we were encouraged to find our own fields of interest and to develop our own areas for research. Perhaps because he was so knowledgeable he did not need to limit our activity to preserve his capacity to direct our work. All of us will remember his wise and searching questions as well as the good conversations that came, often enough, with chocolate and cakes, after our seminar or thesis consultation sessions were over.

Steefel did many things besides teaching. Dur-

ing the war he was coordinator of the Army Specialized Training Program at the University. He was the first director of the Scandinavian Area Program. He was "over used" by the university administration on important university committees, and he carried his share of the committee work in the department. His quiet, unobtrusive manner belied the impact that he had on the governing work of the university. He also was president of the Minnesota AAUP at a difficult period of its existence. In his private life he had many interests: he had a magnificent stamp collection; he summered at Cass Lake where he fished, canoed, studied, and socialized; and he read widely and took an interest in community and political affairs.

Steefel's research took him to the archives and libraries of Germany, Austria, France, Denmark, and Italy; he lived in Europe several years and spent many summers there visiting every important European capital. His research was supported by Harvard fellowships, a Guggenheim fellowship, a Fulbright research grant, and sabbatical leaves from the University of Minnesota. His two books, *The Schleswig-Holstein Question* (1923) and *Bismarck, The Hohenzollern Candidacy, and the Origins of the Franco-German War of 1870* (1962), have become classics. There would have been another book on the diplomacy of the 1860s if a German scholar had not published a volume that almost exactly paralleled Steefel's findings and interpretation. In addition to his books, his essays in historical journals and in the volume honoring G. P. Gooch as well as his numerous reviews provide persuasive evidence of his critical judgment. Minnesotans will also remember that he published several letters and little

essays dealing with that famous Minnesota forgery, the Kensington Runestone.

JOHN B. WOLF
*University of Illinois,
Chicago Circle*

HERMAN N. WEILL died suddenly in his home at the age of fifty-one on March 3, 1976 in Huntington, West Virginia. Weill was born in New York City, received his primary and secondary education in Switzerland, and earned his undergraduate degree in history at the University of Miami. After earning his Ph.D. at the University of Illinois, Urbana-Champaign, in 1959, he taught at State College in Minot, North Dakota, the University of Maryland, the University of Rhode Island, the University of Missouri at Kansas City and St. Louis, and Marshall University. He also served as Dean of Arts and Sciences at Johnson State College in Vermont and Dean of the Graduate School at Marshall University. Herman Weill's research was concentrated in German and European diplomatic history. He was the author of two books: *European Diplomatic History, 1815-1914: Documents and Interpretations* (1972) and *Frederick the Great and Samuel von Cocceji: A Study in the Reform of the Prussian Judicial Administration, 1740-1755* (1961). Weill will be remembered by his colleagues at Marshall University for his incisive conversation, his professionalism, and his devotion to his discipline. Just prior to his death he sponsored and provided the funds for a Prize Essay contest entitled "Why Study History?" This essay contest has been continued as a tribute and memorial.

DAVID R. WOODWARD
Marshall University

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

PAUL A. COHEN and JOHN E. SCHRECKER, editors. *Reform in Nineteenth-Century China*. (Harvard East Asian Monographs, number 72.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press. 1976. Pp. xii, 393. \$9.00.

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STANISŁAW RUSSOCKI, The Parliamentary Systems in 15th-Century Central Europe. STANISŁAW GRZYBOWSKI, The Gentry and the Beginnings of Colonization. JULIUSZ BARDACH, Les statuts lituaniens—codifications de l'époque de la Renaissance. HENRYK ZINS, Sir Philip Sidney and Poland. KAROL GÓSKI, La mentalité polonaise des XVI^e-XVIII^e siècles. JANUSZ TAZBIR, The Cult of St. Isidore the Farmer in Europe. ZBIGNIEW WÓJCIK, Poland and Russia in the 17th Century: Problems of Internal Development. MARIAN HENRYK SEREJSKI, L'aspect européen de la question polonaise. Les réflexions des historiens étrangers sur les partages de la Pologne. JANINA LESKIEWICZ, Studies in Social Structures in 19th-Century Poland. A Report. CELINA BOBINSKA, Poland: Land of Mass Migration (19th and 20th Centuries). TADEUSZ ŁEPKOWSKI, Historia de América Latina: entre una vía de desarrollo "europea" y una tercer-mundista. HENRYK BATOWSKI, 1914 and 1939: Diplomatic Analogies. JANUSZ ZARNOWSKI, East-Central European Societies, 1918-1939: the Polish Example. JERZY W. BOREJSZA, Italian Fascism and East-Central Europe, 1922-1943. LUDWIK HASS, Grandeur et déclin de la franc-maçonnerie en Europe central et orientale, 1926-1938. CZESŁAW MADAJCZYK, L'ingérence du III^e Reich dans la vie culturelle des pays occupés.

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GERHARD JAGSCHITZ, Zur Struktur der NSDAP in Österreich vor dem Juliputsch 1934. ISABELLA ACKERL, Das Kampfbündnis der Nationalsozialistischen Deutschen Arbeiterpartei mit der Grossdeutschen Volkspartei vom 15. Mai 1933. EVERHARD HOLTMANN, Zwischen "Blutschuld" und "Befriedung": Autoritäre Julijustiz. LUDWIG JEDLICKA, Der aussenpolitische Hintergrund der Ereignisse vom Frühsommer 1934 bis Oktober 1934. WALTER HUMMELBERGER, Österreich und die Kleine Entente um die Jahresmitte 1934. RUDOLF NECK, Die Entwicklung der österreichischen Verfassung bis zum Sommer 1934.

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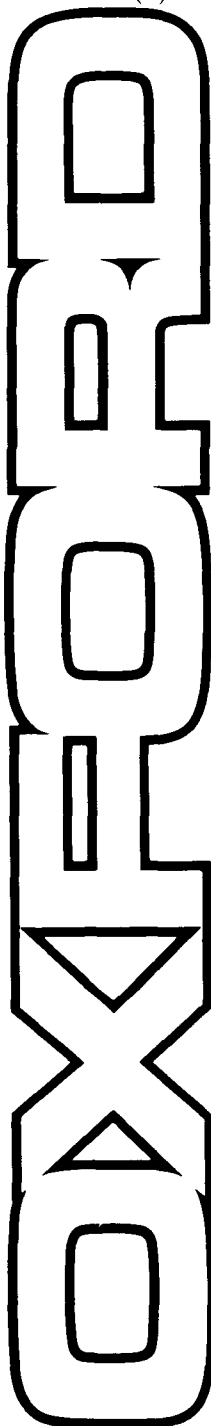
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